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WITHDRAWN

THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

H. H. TURNER, M.A., B.Sc., F.R.A.S.,
AND
A. A. COMMON, F.R.S., Treas. R.A.S.

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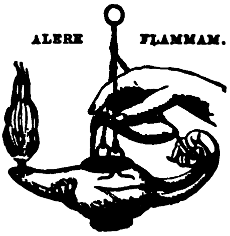
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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 131. 1888. Vol. XI.

INTRODUCTION.

Our readers will see from the cover that the Editorship of the 'Observatory' passes into new hands with the commencement of the year 1888; but it is hoped that the usefulness of the magazine and its completeness as a record of current astronomical events will not be thereby impaired; and it is not intended to make any serious departures from the lines which previous Editors have followed.

The present 'Companion' is very similar to that issued last year. We have again to thank Mr. Denning for the "Meteor Notes," and M. Loewy for the Catalogue and Ephemerides of Variable Stars; while Mr. Marth has kindly furnished Ephemerides for physical observations of Jupiter and Mars, and some valuable information concerning the Satellites of Saturn. The phenomena of Jupiter's Satellites and the elements of occultations are extracted from the English 'Nautical Almanac,' the ephemerides of the Satellites of Mars, Saturn, Uranus, and Neptune from the 'American Nautical Almanac,' corrections having been applied in the case of the last three to secure accordance with Mr. Marth's ephemerides published in the 'Monthly Notices.' The list of Binary Stars has been brought up to date.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign – indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations of stars are reckoned from the *true* north in the direction N. E. S. W., as for double stars, following the precedent of last year.

Greenwich Mean Time is used in all cases, and the astronomical day is reckoned from noon to noon as in previous years.

We have to thank the late Editors for generous and valuable assistance in the compilation of the number; and, following their example, we would ask those who may use the 'Companion' to favour us with any suggestions which may tend to make it more useful to observers.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
1	1	**S**	·000	3 59	20 9	6 37	14 25	+53·0 E.	0 3 40	18 42 20
2	2	M	·003	4 0	20 9	7 49	15 18	+40·9 E.	4 8	46 16
3	3	Tu	·006	4 2	20 8	9 4	16 10	+28·8 E.	4 36	50 13
4	4	W	·008	4 3	20 8	10 20	17 1	+16·6 E.	5 3	54 9
5	5	Th	·011	4 4	20 8	11 38	17 51	+ 4·5 E.	5 30	18 58 6
6	6	F	·014	4 5	20 7	12 55	18 42	− 7·7 E.	5 57	19 2 2
7	7	S	016	4 6	20 7	14 14	19 34	−19·8 E.	6 23	5 59
8	8	**S**	·019	4 7	20 7	15 32	20 28	−32·0 E.	6 49	9 55
9	9	M	·022	4 9	20 6	16 48	21 24	−44·2 E.	7 14	13 52
10	10	Tu	·025	4 10	20 6	18 0	22 22	−56·4 E.	7 39	17 49
11	11	W	·027	4 12	20 5	19 4	23 19	−68·5 E.	8 3	21 45
12	12	Th	·030	4 14	20 4	sets	......	......	8 27	25 42
13	13	F	·033	4 15	20 3	4 36	0 16	+87·2 M.	8 50	29 38
14	14	S	·036	4 17	20 2	5 43	1 10	+75·0 M.	9 13	33 35
15	15	**S**	·038	4 19	20 1	6 52	2 2	+62·8 M.	9 34	37 31
16	16	M	·041	4 20	19 59	8 1	2 50	+50·7 M.	9 55	41 28
17	17	Tu	·044	4 21	19 58	9 8	3 35	+38·5 M.	10 16	45 24
18	18	W	·047	4 23	19 58	10 14	4 19	+26·3 M.	10 35	49 21
19	19	Th	·049	4 24	19 57	11 18	5 1	+14·2 M.	10 54	53 18
20	20	F	·052	4 26	19 56	12 22	5 43	+ 2·0 M.	11 12	19 57 14
21	21	S	·055	4 28	19 55	13 26	6 25	−10·1 M.	11 30	20 1 11
22	22	**S**	·058	4 30	19 53	14 30	7 8	−22·4 M.	11 46	5 7
23	23	M	·060	4 32	19 52	15 34	7 54	−34· M.	12 2	9 4
24	24	Tu	·063	4 33	19 51	16 37	8 42	−46·6 M.	12 17	13 0
25	25	W	·066	4 35	19 50	17 37	9 33	−58·7 M.	12 31	16 57
26	26	Th	·068	4 36	19 48	18 31	10 26	−70·8 M.	12 45	20 53
27	27	F	·071	4 38	19 46	19 19	11 20	−83·0 M.	12 57	24 50
28	28	S	·074	4 40	19 45	rises	12 15	......	13 9	28 47
29	29	**S**	·077	4 42	19 44	5 32	13 10	+72·7 E.	13 20	32 43
30	30	M	·079	4 44	19 43	6 48	14 4	+60·5 E.	13 30	36 40
31	31	Tu	·082	4 46	19 41	8 6	14 56	+48·3 E.	0 13 39	20 40 36

	d	h	m		d	h	m
Last Quarter	**5**	**23**	**43**	**New Moon**	**12**	**20**	**39**
First Quarter	**20**	**16**	**49**	**Full Moon**	**28**	**11**	**19**

Meteor Notes.

There are a considerable number of showers visible during this month, the most important being the *Quadrantids*, R.A. 228°, Decl. +53°. The principal days on which the large meteors, commonly called fireballs, may be expected are Jan. 2, 15, 16, 17, and 28.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
Jan. 9–19	72	+ 4	W. of γ Orionis.	Jan. 1–25	181	+35	Swift; streaks.
Jan. 2–4.........	119	+16	Swift; bright.	Jan. 4–11	220	+13	V. swift; streaks.
Jan. 1-Feb. 6...	133	+48	Near ι Urs. Maj.	Jan. 1–15	221	+42	V. swift; streaks.
Jan. 2–5.........	140	+57	V. swift; short.	Jan. 1–3.........	228	+53	*Quadrantids.*
Jar.–Feb.	167	+ 5	Very swift.	January 28 ...	236	+25	V. swift; streaks.
January 11......	173	+ 9	Swift; streaks.	Jan. 14–17......	295	+53	Slowish, trained.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
32	1	W	·085	4 48	19 40	9 25	15 48	+36·2 E.	0 13 48	20 44 33
33	2	Th	·088	4 49	19 38	10 44	16 39	+24·0 E.	13 55	48 29
34	3	F	·090	4 50	19 36	12 3	17 31	+11·9 E.	14 2	52 26
35	4	S	·093	4 52	19 34	13 20	18 24	− 0·3 E.	14 8	20 56 22
36	5	**S**	·096	4 54	19 32	14 36	19 18	−12·4 E.	14 14	21 0 19
37	6	M	·099	4 56	19 30	15 48	20 14	−24·6 E.	14 18	4 16
38	7	Tu	·101	4 57	19 29	16 54	21 10	−36·8 E.	14 22	8 12
39	8	W	·104	4 59	19 27	17 50	22 6	−49·0 E.	14 25	12 9
40	9	Th	·107	5 0	19 25	18 37	23 0	−61·2 E.	14 27	16 5
41	10	F	·110	5 2	19 24	19 15	23 52	−73·4 E.	14 28	20 2
42	11	S	·112	5 4	19 22	sets	……	……	14 29	23 58
43	12	**S**	·115	5 6	19 20	5 43	0 41	+82·3 M.	14 29	27 55
44	13	M	·118	5 8	19 18	6 51	1 28	+70·2 M.	14 28	31 51
45	14	Tu	·121	5 10	19 16	7 58	2 12	+58·0 M.	14 26	35 48
46	15	W	·123	5 12	19 14	9 3	2 55	+45·8 M.	14 23	39 45
47	16	Th	·126	5 14	19 12	10 8	3 37	+33·7 M.	14 20	43 41
48	17	F	·129	5 16	19 10	11 12	4 19	+21·5 M.	14 16	47 38
49	18	S	·131	5 18	19 8	12 16	5 2	+ 9·4 M.	14 12	51 34
50	19	**S**	·134	5 19	19 7	13 19	5 46	− 2·8 M.	14 6	55 31
51	20	M	·137	5 21	19 5	14 21	6 33	−15·0 M.	14 0	21 59 27
52	21	Tu	·140	5 23	19 3	15 22	7 21	−27·1 M.	13 53	22 3 24
53	22	W	·142	5 25	19 1	16 18	8 12	−39·3 M.	13 46	7 20
54	23	Th	·145	5 27	18 59	17 9	9 5	−51·4 M.	13 38	11 17
55	24	F	·148	5 29	18 56	17 53	10 0	−63·6 M.	13 29	15 14
56	25	S	·151	5 30	18 54	18 31	10 55	−75·8 M.	13 20	19 10
57	26	**S**	·153	5 32	18 52	rises	11 50	……	13 10	23 7
58	27	M	·156	5 34	18 50	5 42	12 45	+79·9 E.	13 0	27 3
59	28	Tu	·159	5 36	18 48	7 4	13 38	+67·7 E.	12 49	31 0
60	29	W	·162	5 37	18 46	8 25	14 32	+55·5 E.	0 12 37	22 34 56

	d h m		d h m
Last Quarter	**4 7 26**	**New Moon**	**11 11 53**
First Quarter	**19 13 59**	**Full Moon**	**26 23 58**

Meteor Notes.

There are no periodical showers of the first order this month. The principa fireball dates are Feb. 2, 10, and 19.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
Feb. 23–25……	30	+35	Near β Trianguli.	February ……	236	+11	Swift; streaks.
Feb. 5–10 ……	74	+43	Near α Aurigæ.	Feb. 17 ………	237	+46	Near υ Herculis.
February ……	111	+62	Slow; yellow.	Feb. 6…………	240	+62	Near θ–77 Draconis.
February ……	120	− 5	Slow; yellow.				
Feb. 5–16 ……	165	+73	Near λ Draconis.	Feb. 13–20……	260	+ 2	V. swift; streaks.
				Feb. 20 ………	263	+36	Rather swift.
Feb. 20 ………	181	+34	V. swift; white.	Feb. 10–Mar. 7.	280	−17	V. swift; streaks.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
61	1	Th	·164	5 39	18 44	9 47	15 25	+43·3 E.	0 12 25	22 38 53
62	2	F	·167	5 41	18 42	11 7	16 19	+31·2 E.	12 12	42 49
63	3	S	·170	5 43	18 40	12 26	17 14	+19·0 E.	11 59	46 46
64	4	**S**	·173	5 45	18 38	13 40	18 10	+ 6·8 E.	11 46	50 42
65	5	M	·175	5 46	18 36	14 48	19 6	− 5·4 E.	11 32	54 39
66	6	Tu	·178	5 48	18 34	15 47	20 1	−17·5 E.	11 18	22 58 36
67	7	W	·181	5 50	18 31	16 36	20 55	−29·7 E.	11 3	23 2 32
68	8	Th	·183	5 51	18 28	17 16	21 47	−41·9 E.	10 48	6 29
69	9	F	·186	5 53	18 26	17 49	22 36	−54·1 E.	10 33	10 25
70	10	S	·189	5 55	18 23	18 16	23 23	−66·3 E.	10 17	14 22
71	11	**S**	·192	5 57	18 31	sets	......	−78·5 E.	10 1	18 18
72	12	M	·194	5 58	18 18	5 45	0 8	......	9 45	22 15
73	13	Tu	·197	6 0	18 16	6 51	0 51	+77·2 M.	9 28	26 11
74	14	W	·200	6 2	18 13	7 56	1 33	+65·0 M.	9 12	30 8
75	15	Th	·202	6 4	18 11	9 0	2 16	+52·8 M.	8 55	34 5
76	16	F	·205	6 6	18 9	10 4	2 58	+40·7 M.	8 37	38 1
77	17	S	·208	6 8	18 7	11 8	3 41	+28·5 M.	8 20	41 58
78	18	**S**	·211	6 10	18 5	12 10	4 26	+16·3 M.	8 2	45 54
79	19	M	·214	6 11	18 3	13 11	5 13	+ 4·1 M.	7 44	49 51
80	20	Tu	·216	6 12	18 1	14 8	6 2	− 8·1 M.	7 26	53 47
81	21	W	·219	6 14	17 59	15 0	6 53	−20·3 M.	7 8	23 57 44
82	22	Th	·222	6 15	17 57	15 46	7 46	−32·5 M.	6 50	0 1 40
83	23	F	·225	6 17	17 54	16 25	8 40	−44·7 M.	6 31	5 37
84	24	S	·227	6 18	17 52	17 0	9 34	−56·9 M.	6 13	9 34
85	25	**S**	·230	6 20	17 50	17 30	10 28	−69·1 M.	5 54	13 30
86	26	M	·233	6 22	17 48	rises	11 22	−81·3 M.	5 36	17 27
87	27	Tu	·236	6 24	17 45	5 56	12 17	......	5 17	21 23
88	28	W	·238	6 26	17 43	7 20	13 12	+74·4 E.	4 59	25 20
89	29	Th	·241	6 28	17 41	8 44	14 7	+62·2 E.	4 40	29 16
90	30	F	·244	6 30	17 38	10 8	15 4	+50·0 E.	4 22	33 13
91	31	S	·246	6 31	17 36	11 27	16 2	+37·8 E.	0 4 4	0 37 9

	d h m		d h m
Last Quarter	**4·15 26**	**New Moon**	**12 4 21**
First Quarter	**20 8 43**	**Full Moon**	**27 10 7**

Meteor Notes.

The most favourable nights for meteor-observations are March 1–4 and 7. Fireballs should be looked for March 1–4, 5–10, and 18.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
Mar. 1–15	50	+48	Near α Aurigæ.	Mar. 2–3	193	+ 1	Near δ Virginis.
Mar. 9–27	98	+46	Near 55 Aurigæ.	Mar. 7	233	−18	Very swift.
Mar. 20	144	+48	Near θ UrsæMaj.	Mar. 7	244	+15	Very swift.
Mar. 13–24 ...	161	+58	Slowish.	March	263	+48	Near β Draconis.
Mar. 3–15	175	+10	Slow; brilliant.	Mar. 28	263	+62	Slowish.
Mar. 8	189	+26	Coma Berenices.	Mar. 1–15	300	+80	Slow; brilliant.

APRIL 1888.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
92	1	**S**	·249	6 33	17 34	12 40	17 0	+25·6 E.	0 3 45	0 41 6
93	2	M	·252	6 35	17 32	13 43	17 57	+13·4 E.	3 27	45 2
94	3	Tu	·255	6 37	17 29	14 36	18 52	+ 1·2 E.	3 10	48 59
95	4	W	·257	6 38	17 27	15 19	19 44	−11·0 E.	2 52	52 56
96	5	Th	·260	6 40	17 24	15 54	20 34	−23·2 E.	2 34	0 56 52
97	6	F	·263	6 41	17 22	16 22	21 21	−35·4 E.	2 17	1 0 49
98	7	S	·266	6 43	17 20	16 46	22 6	−47·6 E.	2 0	4 45
99	8	**S**	·268	6 44	17 18	17 8	22 49	−59·8 E.	1 43	8 42
100	9	M	·271	6 45	17 15	17 28	23 32	−72·0 E.	1 27	12 38
101	10	Tu	·274	6 46	17 13	sets	……	……	1 10	16 35
102	11	W	·277	6 48	17 11	6 51	0 13	+83·6 M.	0 54	20 31
103	12	Th	·279	6 50	17 9	7 55	0 56	+71·4 M.	0 39	24 28
104	13	F	·282	6 52	17 7	8 59	1 39	+59·2 M.	0 23	28 25
105	14	S	·285	6 53	17 5	10 2	2 23	+47·0 M.	0 0 8	32 21
106	15	**S**	·288	6 55	17 2	11 4	3 9	+34·8 M.	23 59 54	36 18
107	16	M	·290	6 57	17 0	12 2	3 57	+22·6 M.	59 39	40 14
108	17	Tu	·293	6 59	16 58	12 55	4 47	+10·4 M.	59 25	44 11
109	18	W	·296	7 0	16 56	13 42	5 38	− 1·8 M.	59 12	48 7
110	19	Th	·298	7 2	16 55	14 23	6 29	−14·0 M.	58 58	52 4
111	20	F	·301	7 4	16 53	14 58	7 22	−26·2 M.	58 46	56 0
112	21	S	·304	7 6	16 51	15 29	8 14	−38·4 M.	58 33	1 59 57
113	22	**S**	·307	7 8	16 49	15 56	9 7	−50·6 M.	58 21	2 3 54
114	23	M	·309	7 10	16 47	16 22	10 0	−62·8 M.	58 10	7 50
115	24	Tu	·312	7 11	16 45	rises	10 54	−75·0 M.	57 58	11 47
116	25	W	·315	7 13	16 43	6 11	11 50	……	57 48	15 43
117	26	Th	·318	7 14	16 41	7 37	12 47	+80·6 E.	57 38	19 40
118	27	F	·320	7 16	16 39	9 1	13 46	+68·4 E.	57 28	23 36
119	28	S	·323	7 17	16 37	10 21	14 46	+56·2 E.	57 19	27 33
120	29	**S**	·326	7 19	16 35	11 32	15 46	+44·0 E.	57 10	31 29
121	30	M	·329	7 21	16 33	12 32	16 44	+31·8 E.	23 57 2	2 35 26

	d	h	m		d	h	m
Last Quarter	3	0	41	**New Moon**	10	21	8
First Quarter	18	23	52	**Full Moon**	25	18	22

Meteor Notes.

There are several good showers which may usually be expected this month. The *Lyrids* form the most interesting. The chief fireball dates are April 2, 6, 10, 20, and 25–30. The *Lyrid* radiant is 269° +33°.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
April 9–12 ……	162	+59	Near β Ursæ Maj.	April 9–12 ……	249	+51	Near τ Herculis.
April … ……	174	+ 7	Slow ; bright.	April 9–26 ……	255	+37	Very swift.
April–May ……	208	+58	Slow ; bright.	April 19–20 …	269	+33	*Lyrids* ; swift.
April–May ……	228	− 4	Rather slow.	April 18–26 …	272	+21	Swift ; short.
April 17–25 …	231	+17	Very swift.	April 19–20 …	299	+24	Very swift.
April 27–May 5	234	+10	Swift.	April 1–12 ……	304	+12	Slow ; brilliant.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
122	1	Tu	·331	7 23	16 31	13 20	17 39	+19·6 E.	23 56 55	2 39 23
123	2	W	·334	7 24	16 30	13 58	18 31	+ 7·4 E.	56 48	43 19
124	3	Th	·337	7 26	16 29	14 28	19 20	− 4·8 E.	56 42	47 16
125	4	F	·340	7 27	16 28	14 53	20 5	−17·0 E.	56 36	51 12
126	5	S	·342	7 29	16 27	15 15	20 49	−29·2 E.	56 31	55 9
127	6	**S**	·345	7 30	16 26	15 35	21 31	−41·4 E.	56 26	2 59 5
128	7	M	·348	7 31	16 24	15 54	22 13	−53·6 E.	56 22	3 3 2
129	8	Tu	·350	7 33	16 22	16 14	22 55	−65·8 E.	56 18	6 58
130	9	W	·353	7 34	16 21	16 35	23 37	−78·1 E.	56 15	10 55
131	10	Th	·356	7 36	16 19	sets	……	……	56 13	14 52
132	11	F	·359	7 38	16 18	7 55	0 21	+77·6 M.	56 11	18 48
133	12	S	·361	7 39	16 16	8 57	1 7	+65·4 M.	56 10	22 45
134	13	**S**	·364	7 41	16 14	9 57	1 54	+53·2 M.	56 10	26 41
135	14	M	·367	7 42	16 12	10 53	2 43	+41·0 M.	56 10	30 38
136	15	Tu	·370	7 44	16 11	11 42	3 34	+28·8 M.	56 10	34 34
137	16	W	·372	7 45	16 10	12 24	4 25	+16·6 M.	56 11	38 31
138	17	Th	·375	7 47	16 8	13 0	5 16	+ 4·4 M.	56 13	42 27
139	18	F	·378	7 48	16 6	13 31	6 7	− 7·8 M.	56 15	46 24
140	19	S	·381	7 49	16 5	13 58	6 58	−20·1 M.	56 18	50 21
141	20	**S**	·383	7 50	16 4	14 23	7 49	−32·4 M.	56 21	54 17
142	21	M	·386	7 51	16 3	14 48	8 40	−44·6 M.	56 25	3 58 14
143	22	Tu	·389	7 53	16 2	15 13	9 33	−56·8 M.	56 29	4 2 10
144	23	W	·392	7 55	16 0	15 41	10 28	−69·0 M.	56 34	6 7
145	24	Th	·394	7 57	15 58	16 14	11 26	−81·3 M.	56 39	10 3
146	25	F	·397	7 58	15 57	rises	12 26	……	56 45	14 0
147	26	S	·399	7 59	15 56	9 10	13 28	+74·3 E.	56 51	17 56
148	27	**S**	·402	8 0	15 55	10 18	14 28	+62·0 E.	56 58	21 53
149	28	M	·405	8 1	15 54	11 13	15 27	+49·8 E.	57 5	25 50
150	29	Tu	·408	8 2	15 52	11 57	16 23	+37·6 E.	57 13	29 46
151	30	W	·411	8 3	15 51	12 31	17 14	+25·4 E.	57 21	33 43
152	31	Th	·413	8 4	15 51	12 59	18 2	+13·2 E.	23 57 29	4 37 39

	d	h	m		d	h	m
Last Quarter	2	11	47	**New Moon**	10	13	24
First Quarter	18	11	5	**Full Moon**	25	1	40

Meteor Notes.

There are no great periodical showers in this month. Fireball dates:— May 1, 13, 20, 21, and 31.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
May 3–15 ……	170	−10	Very slow.	May 15 ………	294	± 0	Very swift.
May 7–18 ……	231	+27	Slowish; faint; short.	May 30 & June .	303	+24	Swift.
May 1…………	239	+46	Swift; short; faint.	May 11–18……	314	+15	V. swift; streaks.
May 1–7………	254	−21	Slow; long.	May–June ……	329	+48	V. swift.
May 1–14 ……	*262*	*+64*	Rather slow.	May 29–June 4	333	+27	V. swift; streaks.
May 30 ………	*290*	*+60*	*Short*; slow.	May 2–6………	337	− 2½	Swift; v. long; streaks.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
153	1	F	·416	8 5	15 51	13 21	18 47	+ 1·0 E.	23 57 38	4 41 36
154	2	S	·419	8 6	15 50	13 42	19 30	−11·3 E.	57 48	45 32
155	3	**S**	·422	8 7	15 50	14 2	20 12	−23·5 E.	57 58	49 29
156	4	M	·424	8 8	15 49	14 20	20 53	−35·7 E.	58 8	53 25
157	5	Tu	·427	8 9	15 48	14 41	21 35	−47·9 E.	58 18	4 57 22
158	6	W	·430	8 10	15 47	15 3	22 19	−60·1 E.	58 29	5 1 19
159	7	Th	·433	8 11	15 47	15 29	23 4	−72·4 E.	58 40	5 15
160	8	F	·435	8 11	15 46	15 59	23 51	−84·6 E.	58 52	9 12
161	9	S	·438	8 12	15 46	sets	......	......	59 4	13 8
162	10	**S**	·441	8 13	15 45	8 49	0 40	+71·1 M.	59 15	17 5
163	11	M	·444	8 14	15 45	9 41	1 31	+58·9 M.	59 28	21 1
164	12	Tu	·446	8 15	15 45	10 26	2 22	+46·6 M.	59 40	24 58
165	13	W	·449	8 16	15 45	11 4	3 13	+34·4 M.	23 59 53	28 54
166	14	Th	·452	8 16	15 44	11 36	4 4	+22·2 M.	0 0 5	32 51
167	15	F	·455	8 16	15 44	12 4	4 54	+10·0 M.	0 18	36 48
168	16	S	·457	8 16	15 44	12 29	5 44	− 2·3 M.	0 31	40 44
169	17	**S**	·460	8 17	15 44	12 52	6 34	−14·5 M.	0 44	44 41
170	18	M	·463	8 17	15 44	13 16	7 24	−26·8 M.	0 57	48 37
171	19	Tu	·465	8 18	15 44	13 41	8 16	−39·0 M.	1 10	52 34
172	20	W	·468	8 18	15 45	14 10	9 11	−51·2 M.	1 22	5 56 30
173	21	Th	·471	8 18	15 45	14 44	10 8	−63·4 M.	1 35	6 0 27
174	22	F	·474	8 19	15 45	15 27	11 8	−75·7 M.	1 48	4 23
175	23	S	·476	8 19	15 45	rises	12 9	......	2 1	8 20
176	24	**S**	·479	8 19	15 46	8 59	13 10	+79·9 E.	2 14	12 17
177	25	M	·482	8 18	15 46	9 49	14 8	+67·7 E.	2 26	16 13
178	26	Tu	·485	8 18	15 46	10 29	15 3	+55·4 E.	2 39	20 10
179	27	W	·487	8 18	15 46	11 0	15 54	+43·2 E.	2 51	24 6
180	28	Th	·490	8 18	15 47	11 26	16 41	+31·0 E.	3 3	28 3
181	29	F	·493	8 18	15 47	11 48	17 26	+18·8 E.	3 15	31 59
182	30	S	·496	8 18	15 49	12 8	18 9	+ 6·5 E.	0 3 27	6 35 56

	d	h	m		d	h	m		d	h	m
Last Quarter	1	0	53	**New Moon**	9	4	34	**Last Quarter**	30	15	53
First Quarter	16	18	50	**Full Moon**	23	9	8				

Meteor Notes.

There is so little real darkness during the nights at this season of the year that the work of observing meteors is rendered somewhat difficult. A fair number may, however, usually be seen. Fireball dates:—June 4 and 9.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
June 1–29	168	+55	Near β Urs. Maj.	June 26–July 6	294	+39	Slow.
June 2	227	+30	Near β Coronæ.	June 28	305	+ 9	Near ε Delphini.
June 3–17	249	−20	Fireballs.	June	315	+60	Swift; streaks.
June 25–30 ...	253	+47	Swift.	June	319	+32	Near ζ Cygni.
June 3–13	261	+ 5	Slowish.	June 10–28 ...	335	+57	Swift.
June 12–21......	285	+23	Slowish.	June 11–13......	345	± 0	Very swift.

JULY 1888.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
183	1	**S**	·498	8 17	15 50	12 27	18 51	− 5·7 E.	0 3 38	6 39 52
184	2	M	·501	8 17	15 51	12 47	19 33	−18·0 E.	3 50	43 49
185	3	Tu	·504	8 16	15 52	13 8	20 15	−30·2 E.	4 1	47 46
186	4	W	·507	8 16	15 53	13 32	21 0	−42·5 E.	4 12	51 42
187	5	Th	·509	8 15	15 54	14 0	21 46	−54·7 E.	4 22	55 39
188	6	F	·512	8 15	15 55	14 34	22 35	−67·0 E.	4 32	6 59 35
189	7	S	·515	8 14	15 56	15 16	23 25	−79·2 E.	4 42	7 3 32
190	8	**S**	·518	8 14	15 57	sets	……	……	4 51	7 28
191	9	M	·520	8 13	15 58	8 24	0 17	+76·5 M.	5 0	11 25
192	10	Tu	·523	8 13	15 59	9 5	1 9	+64·2 M.	5 9	15 22
193	11	W	·526	8 12	16 0	9 39	2 1	+51·9 M.	5 17	19 18
194	12	Th	·528	8 11	16 1	10 9	2 52	+39·6 M.	5 24	23 15
195	13	F	·531	8 10	16 2	10 35	3 42	+27·3 M.	5 31	27 11
196	14	S	·534	8 9	16 3	10 58	4 31	+15·0 M.	5 38	31 8
197	15	**S**	·537	8 8	16 4	11 21	5 21	+ 2·7 M.	5 44	35 4
198	16	M	·539	8 7	16 5	11 45	6 11	− 9·7 M.	5 50	39 1
199	17	Tu	·542	8 6	16 6	12 11	7 3	−22·1 M.	5 55	42 57
200	18	W	·545	8 5	16 7	12 42	7 57	−34·4 M.	5 59	46 54
201	19	Th	·548	8 4	16 9	13 20	8 55	−46·7 M.	6 3	50 51
202	20	F	·550	8 3	16 10	14 7	9 54	−58·9 M.	6 7	54 47
203	21	S	·553	8 2	16 11	15 4	10 54	−71·1 M.	6 10	7 58 44
204	22	**S**	·556	8 0	16 12	rises	11 53	……	6 12	8 2 40
205	23	M	·559	7 58	16 14	8 23	12 49	+84·7 E.	6 13	6 37
206	24	Tu	·561	7 56	16 15	8 58	13 42	+72·6 E.	6 14	10 33
207	25	W	·564	7 54	16 17	9 27	14 32	+60·5 E.	6 15	14 30
208	26	Th	·567	7 53	16 19	9 50	15 19	+48·4 E.	6 15	18 26
209	27	F	·570	7 51	16 21	10 11	16 3	+36·3 E.	6 14	22 23
210	28	S	·572	7 50	16 23	10 31	16 46	+24·2 E.	6 13	26 20
211	29	**S**	·575	7 49	16 24	10 51	17 28	+12·1 E.	6 11	30 16
212	30	M	·578	7 47	16 26	11 12	18 11	− 0·1 E.	6 9	34 13
213	31	Tu	·580	7 46	16 27	11 34	18 54	−12·3 E.	0 6 6	8 38 9

	d	h	m		d	h	m
New Moon	8	18	17	**First Quarter**	16	0	13
Full Moon	22	17	45	**Last Quarter**	30	8	30

Meteor Notes.

This month is usually rich in periodical showers, though the long-continued twilight and the shortness of the nights still render observation difficult. The *Aquarids*, R.A. 340°, Dec. −13°, max. July 28, form the chief display. Fireball dates:—July 5, 12, 16, 18, 20, and 30.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
July 27–Aug.…	7	+32	Swift; streaks.	July 9–14 ……	271	+21	Very slow.
July 20–Aug. 4	20	+58	V. swift; streaks.	July 9–13 ……	280	−14	Very slow.
	11	+48	*Perseids*; swift;	July 9–14 ……	329	+36	Swift; red strks.
July 13–31…	t	o	streaks.	July 16–31……	335	+49	Swift; short.
	33	+55		July 25–31……	340	−13	*Aquarids.*
July, Aug., Sept.	48	+43	V. swift; streaks.	July–Aug. ……	350	+52	Very swift.
July 14–31……	269	+49	Swift.	July 9–12 ……	352	+38	Swift.

AUGUST 1888.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
214	1	W	·583	7 44	16 28	12 1	19 40	−24·5 E.	0 6 2	8 42 6
215	2	TH	·586	7 43	16 30	12 32	20 27	−36·7 E.	5 58	46 2
216	3	F	·589	7 42	16 32	13 10	21 17	−48·9 E.	5 53	49 59
217	4	S	·591	7 40	16 33	13 57	22 8	−61·2 E.	5 48	53 55
218	5	**S**	·594	7 38	16 35	14 53	23 1	−73·4 E.	5 42	8 57 52
219	6	M	·597	7 36	16 36	15 57	23 54	−85·7 E.	5 35	9 1 49
220	7	TU	·600	7 34	16 37	sets	……	……	5 28	5 45
221	8	W	·602	7 32	16 39	8 11	0 46	+69·9 M.	5 20	9 42
222	9	TH	·605	7 31	16 40	8 38	1 37	+57·7 M.	5 12	13 38
223	10	F	·608	7 29	16 42	9 3	2 28	+45·5 M.	5 3	17 35
224	11	S	·611	7 27	16 44	9 27	3 18	+33·3 M.	4 54	21 31
225	12	**S**	·613	7 25	16 45	9 50	4 8	+21·1 M.	4 44	25 28
226	13	M	·616	7 23	16 46	10 15	5 0	+ 8·9 M.	4 33	29 24
227	14	TU	·619	7 21	16 47	10 44	5 53	− 3·3 M.	4 22	33 21
228	15	W	·622	7 19	16 49	11 19	6 48	−15·6 M.	4 10	37 18
229	16	TH	·624	7 17	16 51	12 1	7 45	−27·8 M.	3 58	41 14
230	17	F	·627	7 15	16 52	12 53	8 43	−40·0 M.	3 45	45 11
231	18	S	·630	7 13	16 53	13 54	9 41	−52·3 M.	3 32	49 7
232	19	**S**	·633	7 11	16 55	15 3	10 38	−64·5 M.	3 18	53 4
233	20	M	·635	7 9	16 57	16 15	11 32	−76·7 M.	3 4	9 57 0
234	21	TU	·638	7 7	16 59	rises	12 23	……	2 49	10 0 57
235	22	W	·641	7 5	17 1	7 52	13 10	+79·0 E.	2 34	4 53
236	23	TH	·643	7 3	17 2	8 14	13 56	+66·8 E.	2 18	8 50
237	24	F	·646	7 1	17 3	8 35	14 40	+54·6 E.	2 2	12 46
238	25	S	·649	6 59	17 5	8 54	15 22	+42·4 E.	1 46	16 43
239	26	**S**	·652	6 57	17 7	9 14	16 5	+30·2 E.	1 29	20 40
240	27	M	·654	6 55	17 8	9 36	16 48	+17·9 E.	1 12	24 36
241	28	TU	·657	6 53	17 10	10 1	17 33	+ 5·7 E.	0 54	28 33
242	29	W	·660	6 51	17 12	10 30	18 19	− 6·6 E.	0 36	32 29
243	30	TH	·663	6 49	17 14	11 5	19 8	−18·8 E.	0 18	36 26
244	31	F	·665	6 46	17 15	11 47	19 58	−31·0 E.	0 0 0	10 40 23

	d	h	m		d	h	m
New Moon	7	6	21	**First Quarter**	14	4	44
Full Moon	21	4	20	**Last Quarter**	29	2	18

Meteor Notes.

August is usually the richest month in the year for meteoric displays, the *Perseids*, on Aug. 10, when the radiant is at 44° +56°, being the principal shower. Meteors from neighbouring radiants are usually observed from Aug. 6 to Aug. 12. Fireball dates:—Aug. 4, 10, 16, and 25.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
Aug.–Sept. ……	6°	+11°	Swift.	Aug. 3–23 ……	61°	+49°	V. swift; streaks.
August ………	25	+42	Swift; streaks.	Aug. 7–22 ……	73	+41	Swift; streaks.
Aug., Sept., Oct.	30	+36	58 Andromedæ.	Aug. 6–12 ……	96	+71	Slow.
Aug. 1–19 …	34 to 68	+56 to +57	*Perseids*; swift; streaks.	Aug. 21–25 ……	291	+60	Slow; trained; brilliant.
				August ………	291	+70	Swift; short.
Aug. 8–11 ……	44	+25	Swift; streaks.	July 27–Aug. 17	292	+52	Rather slow.
Aug. 20–24 ……	54	+71	Swift; streaks.	Aug.–Sept. …	335	+58	Swift.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Termi-nator.		
				h m	h m	h m	h m	°	h m s	h m s
245	1	S	·668	6 44	17 16	12 39	20 49	−43·3 E.	23 59 41	10 44 19
246	2	S	·671	6 42	17 18	13 39	21 42	−55·5 E.	59 22	48 15
247	3	M	·674	6 40	17 20	14 47	22 35	−67·7 E.	59 2	52 12
248	4	Tu	·676	6 37	17 21	16 1	23 27	−79·9 E.	58 43	10 56 9
249	5	W	·679	6 35	17 23	sets	......	......	58 23	11 0 5
250	6	Th	·682	6 32	17 25	7 5	0 19	+75·8 M.	58 3	4 2
251	7	F	·685	6 29	17 26	7 30	1 11	+63·6 M.	57 43	7 58
252	8	S	·687	6 27	17 27	7 54	2 2	+51·4 M.	57 22	11 55
253	9	S	·690	6 25	17 29	8 19	2 55	+39·2 M.	57 2	15 51
254	10	M	·693	6 22	17 31	8 46	3 48	+27·0 M.	56 41	19 48
255	11	Tu	·695	6 20	17 32	9 20	4 44	+14·7 M.	56 20	23 44
256	12	W	·698	6 18	17 33	9 59	5 40	+ 2·5 M.	55 59	27 41
257	13	Th	·701	6 16	17 35	10 48	6 38	− 9·7 M.	55 38	31 38
258	14	F	·704	6 14	17 36	11 45	7 36	−21·9 M.	55 17	35 34
259	15	S	·706	6 12	17 38	12 51	8 32	−34·1 M.	54 56	39 31
260	16	S	·709	6 10	17 40	14 0	9 25	−46·3 M.	54 35	43 27
261	17	M	·712	6 7	17 42	15 12	10 16	−58·5 M.	54 13	47 24
262	18	Tu	·715	6 5	17 43	16 23	11 4	−70·7 M.	53 52	51 20
263	19	W	·717	6 2	17 45	rises	11 50	......	53 31	55 17
264	20	Th	·720	6 0	17 46	6 39	12 34	+85·0 E.	53 10	11 59 13
265	21	F	·723	5 58	17 48	6 58	13 17	+72·8 E.	52 49	12 3 10
266	22	S	·726	5 56	17 49	7 17	14 0	+60·6 E.	52 28	7 7
267	23	S	·728	5 54	17 51	7 38	14 43	+48·4 E.	52 7	11 3
268	24	M	·731	5 52	17 53	8 2	15 27	+36·2 E.	51 46	15 0
269	25	Tu	·734	5 50	17 55	8 29	16 13	+24·0 E.	51 25	18 56
270	26	W	·737	5 47	17 56	9 1	17 0	+11·8 E.	51 5	22 53
271	27	Th	·739	5 45	17 58	9 39	17 49	− 0·4 E.	50 45	26 49
272	28	F	·742	5 43	17 59	10 26	18 39	−12·6 E.	50 25	30 46
273	29	S	·745	5 41	18 1	11 22	19 30	−24·8 E.	50 6	34 42
274	30	S	·747	5 40	18 3	12 26	20 22	−37·0 E.	23 49 46	12 38 39

●	d	h	m		d	h	m
New Moon	5	16	56	First Quarter	12	10	0
Full Moon	19	17	24	Last Quarter	27	20	30

Meteor Notes.

Except for fireballs, which may be looked for on September 1, 9, 15, 16, and 24, the month is not a particularly important one for meteors. Sept. 10 is the principal date.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
Aug., Sept., Oct.	31	+18	S. of α Arietis.	Sept. 14–26 ...	98	+43	V.swift; streaks.
Aug.–Sept.	42	+39	Swift; streaks.	Sept.–Oct.	105	+51	Very swift.
Sept. 1–23	62	+37	Swift; streaks.	Aug.–Sept.	282	+42	Swift; bright.
Sept. 7–24	64	+22	Swift; streaks.	Sept.–Oct.	292	+70	Rather swift.
Sept. 4–22	73	+14	Swift; streaks.	Sept. 1	306	+54	Swift; bright.
Sept. 21, Oct. 2.	74	+41	Swift; streaks.	Aug.–Sept.	346	± 0	Slow; bright.
Sept. 15–19 ...	88	+18	V.swift; streaks.	Sept. 1–15	354	+38	Very swift.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
275	1	M	·750	5 38	18 5	13 36	21 14	−49·2 E.	23 49 27	12 42 35
276	2	Tu	·753	5 35	18 7	14 51	22 6	−61·4 E.	49 8	46 32
277	3	W	·756	5 32	18 9	16 10	22 58	−73·6 E.	48 50	50 29
278	4	Th	·758	5 30	18 10	17 31	23 50	−85·8 E.	48 32	54 25
279	5	F	·761	5 27	18 12	sets	......	......	48 14	12 58 22
280	6	S	·764	5 25	18 14	6 19	0 43	+69·9 M.	47 57	13 2 18
281	7	**S**	·767	5 22	18 16	6 6	1 38	+57·7 M.	47 40	6 15
282	8	M	·769	5 20	18 17	7 18	2 35	+45·5 M.	47 24	10 11
283	9	Tu	·772	5 18	18 19	7 56	3 33	+33·3 M.	47 8	14 8
284	10	W	·775	5 15	18 20	8 42	4 32	+21·1 M.	46 52	18 4
285	11	Th	·778	5 13	18 22	9 38	5 31	+ 8·9 M.	46 37	22 1
286	12	F	·780	5 11	18 24	10 42	6 28	− 3·2 M.	46 22	25 58
287	13	S	·783	5 8	18 25	11 51	7 22	−15·4 M.	46 8	29 54
288	14	**S**	·786	5 6	18 27	13 1	8 13	−27·6 M.	45 55	33 51
289	15	M	·789	5 4	18 28	14 12	9 2	−39·8 M.	45 41	37 47
290	16	Tu	·791	5 2	18 30	15 22	9 48	−52·0 M.	45 29	41 44
291	17	W	·794	5 0	18 31	16 30	10 32	−64·2 M.	45 17	45 40
292	18	Th	·797	4 58	18 32	17 37	11 14	−76·4 M.	45 6	49 37
293	19	F	·800	4 56	18 34	rises	11 57	......	44 55	53 33
294	20	S	·802	4 54	18 36	5 43	12 40	+79·3 E.	44 45	13 57 30
295	21	**S**	·805	4 52	18 38	6 4	13 23	+67·1 E.	44 35	14 1 27
296	22	M	·808	4 50	18 40	6 29	14 8	+54·9 E.	44 26	5 23
297	23	Tu	·810	4 47	18 42	6 59	14 55	+42·7 E.	44 18	9 20
298	24	W	·813	4 45	18 44	7 35	15 43	+30·5 E.	44 11	13 16
299	25	Th	·816	4 43	18 46	8 18	16 32	+18·3 E.	44 4	17 13
300	26	F	·819	4 41	18 48	9 9	17 22	+ 6·2 E.	43 59	21 9
301	27	S	·821	4 39	18 50	10 9	18 12	− 6·0 E.	43 53	25 6
302	28	**S**	·824	4 37	18 51	11 15	19 2	−18·2 E.	43 49	29 2
303	29	M	·827	4 36	18 53	12 26	19 53	−30·3 E.	43 45	32 59
304	30	Tu	·830	4 34	18 55	13 42	20 43	−42·5 E.	43 43	36 56
305	31	W	·832	4 32	18 57	15 0	21 34	−54·7 E.	23 43 41	14 40 52

	d	h	m		d	h	m
New Moon	5	2	34	**First Quarter**	11	17	29
Full Moon	19	9	9	**Last Quarter**	27	13	56

Meteor Notes.

The chief shower of October is the *Orionids*, R.A. 90°, Dec. +15°; but there are a large number of active radiants visible during the month, the principal displays taking place on Oct. 1 and Oct. 16–23. Fireball dates:—Oct. 10, 14, 18, 24, and 29.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
Oct. 7–20	31	+ 9	Slow; trained.	Oct.–Nov.	105	+12	Swift; streaks.
Oct. 12–21	40	+20	Swift.	Sept. 25–Oct. 28	105	+22	Swift; streaks.
Oct. 6–16	42	+55	Slow.	Oct. 2–19	133	+79	Swift; streaks.
Oct., Nov., Dec.	78	+32	Swift.	Oct.–Nov.	133	+21	Very swift.
Oct. 15–24	90	+15	*Orionids.*	Oct. 2	225	+52	Slow; brilliant.
Oct. 6–12	103	+33	Swift; streaks.	Oct. 2–20	310	+77	Slow; faint.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
306	1	Th	·835	4 31	18 59	16 22	22 27	−66·9 E.	23 43 40	14 44 49
307	2	F	·838	4 29	19 0	17 46	23 21	−79·1 E.	43 39	48 45
308	3	S	·841	4 27	19 2	sets	……	……	43 40	52 42
309	4	**S**	·843	4 25	19 4	5 12	0 18	+76·7 M.	43 41	14 56 38
310	5	M	·846	4 24	19 5	5 48	1 17	+64·5 M.	43 43	15 0 35
311	6	Tu	·849	4 23	19 7	6 32	2 18	+52·3 M.	43 46	4 31
312	7	W	·852	4 22	19 9	7 26	3 20	+40·2 M.	43 50	8 28
313	8	Th	·854	4 20	19 10	8 29	4 20	+28·0 M.	43 54	12 24
314	9	F	·857	4 19	19 12	9 39	5 17	+15·8 M.	44 0	16 21
315	10	S	·860	4 17	19 14	10 51	6 11	+ 3·6 M.	44 6	20 18
316	11	**S**	·862	4 16	19 16	12 3	7 0	− 8·5 M.	44 13	24 14
317	12	M	·865	4 14	19 18	13 13	7 47	−20·7 M.	44 21	28 11
318	13	Tu	·868	4 12	19 19	14 21	8 31	−32·8 M.	44 30	32 7
319	14	W	·871	4 11	19 21	15 28	9 14	−45·0 M.	44 39	36 4
320	15	Th	·873	4 10	19 23	16 34	9 56	−57·2 M.	44 50	40 0
321	16	F	·876	4 9	19 25	17 40	10 38	−69·4 M.	45 1	43 57
322	17	S	·879	4 8	19 27	18 46	11 21	−81·6 M.	45 13	47 53
323	18	**S**	·882	4 7	19 28	rises.	12 6	……	45 26	51 50
324	19	M	·884	4 6	19 30	5 1	12 52	+74·0 E.	45 40	55 47
325	20	Tu	·887	4 4	19 31	5 34	13 39	+61·8 E.	46 54	15 59 43
326	21	W	·890	4 3	19 33	6 14	14 28	+49·7 E.	46 10	16 3 40
327	22	Th	·893	4 2	19 34	7 2	15 17	+37·5 E.	46 26	7 36
328	23	F	·895	4 0	19 36	7 58	16 7	+25·4 E.	46 43	11 33
329	24	S	·898	3 59	19 37	9 1	16 57	+13·2 E.	47 1	15 29
330	25	**S**	·901	3 58	19 39	10 9	17 45	+ 1·1 E.	47 19	19 26
331	26	M	·904	3 57	19 40	11 21	18 34	−11·1 E.	47 38	23 23
332	27	Tu	·906	3 56	19 42	12 36	19 23	−23·3 E.	47 58	27 19
333	28	W	·909	3 55	19 44	13 53	20 13	−35·4 E.	48 19	31 16
334	29	Th	·912	3 54	19 45	15 13	21 4	−47·6 E.	48 41	35 12
335	30	F	·915	3 53	19 47	16 37	21 58	−59·7 E.	23 49 3	16 39 9

	d	h	m		d	h	m
New Moon	3	12	2	**First Quarter**	10	4	16
Full Moon	18	3	16	**Last Quarter**	26	5	21

Meteor Notes.

The two most interesting November showers are the *Leonids*, R.A. 149°, Dec. +22°, max. Nov. 14, and the *Andromedes*, R.A. 24°, Dec. +44°, max. Nov. 27; but no great display of either is expected this year. Other well-marked showers are the *Taurids*, R.A. 60°, Dec. +20°, and *Leo Minorids*, R.A. 155°, Dec. +40°. Fireball dates:—Nov. 1, 5, 14, 20, and 29.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
Nov. 24–30 …	24	+44	V. slow; trained.	Oct., Nov., Dec.	142	+27	Very swift.
Oct. 30–Nov. 1	43	+22	Slow; brilliant.	Oct., Nov., Dec.	143	+49	Very swift.
Oct.–Nov. ……	54	+71	Swift.	Nov. 11–15… ..	149	+22	Swift; streaks.
Nov. 2–3………	55	+ 9	Slow; brilliant.	Nov. 13–28……	155	+40	Swift; streaks.
Oct. 25–Nov. 28	60	+20	Slow; bright.	Nov. 14–17……	166	+31	Swift; streaks.
Nov. 27–30……	60	+49	Very swift.	Nov. 30–Dec. 4	194	+43	V. swift; streaks.
Nov. 13–16……	125	+41	Swift; streaks.	Nov. 1–6 ……	348	+52	Rather slow.

Day.			Fraction of year.	Sun.		Moon.			Mean Time at Apparent Noon.	Sidereal Time at Mean Noon.
Year.	Month.	Week.		Sets.	Rises.	Rises.	Souths.	Long. of Terminator.		
				h m	h m	h m	h m	°	h m s	h m s
336	1	S	·917	3 52	19 48	18 3	22 56	−71·8 E.	23 49 25	16 43 5
337	2	**S**	·920	3 52	19 49	19 28	23 57	……	49 49	47 2
338	3	M	·923	3 51	19 51	sets	……	+83·8 M.	50 13	50 58
339	4	Tu	·925	3 51	19 52	5 7	1 0	+71·6 M.	50 38	54 55
340	5	W	·928	3 51	19 53	6 8	2 3	+59·4 M.	51 3	16 58 52
341	6	Th	·931	3 50	19 54	7 18	3 4	+47·3 M.	51 29	17 2 48
342	7	F	·934	3 50	19 56	8 32	4 1	+35·1 M.	51 55	6 45
343	8	S	·936	3 50	19 57	9 47	4 54	+23·0 M.	52 21	10 41
344	9	**S**	·939	3 49	19 58	11 0	5 43	+10·8 M.	52 49	14 38
345	10	M	·942	3 49	19 59	12 10	6 29	− 1·3 M.	53 16	18 34
346	11	Tu	·945	3 49	20 0	13 18	7 13	−13·5 M.	53 44	22 31
347	12	W	·947	3 49	20 1	14 25	7 55	−25·6 M.	54 12	26 27
348	13	Th	·950	3 49	20 2	15 31	8 37	−37·8 M.	54 41	30 24
349	14	F	·953	3 49	20 3	16 36	9 19	−49·9 M.	55 9	34 21
350	15	S	·956	3 49	20 4	17 41	10 3	−62·1 M.	55 38	38 17
351	16	**S**	·958	3 49	20 5	18 44	10 49	−74·3 M.	56 8	42 14
352	17	M	·961	3 50	20 5	19 44	11 36	……	56 37	46 10
353	18	Tu	·964	3 50	20 6	rises	12 24	+81·3 E.	57 7	50 7
354	19	W	·967	3 50	20 6	4 58	13 14	+69·1 E.	57 37	54 3
355	20	Th	·969	3 51	20 6	5 53	14 4	+57·0 E.	58 6	17 58 0
356	21	F	·972	3 51	20 7	6 53	14 54	+44·8 E.	58 36	18 1 56
357	22	S	·975	3 51	20 7	7 59	15 43	+32·7 E.	59 6	5 53
358	23	**S**	·977	3 52	20 7	9 9	16 31	+20·5 E.	23 59 36	9 50
359	24	M	·980	3 53	20 8	10 21	17 18	+ 8·4 E.	0 0 6	13 46
360	25	Tu	·983	3 53	20 8	11 35	18 6	− 3·7 E.	0 36	17 43
361	26	W	·986	3 54	20 8	12 52	18 54	−15·8 E.	1 6	21 39
362	27	Th	·988	3 55	20 8	14 10	19 45	−28·0 E.	1 35	25 36
363	28	F	·991	3 56	20 9	15 32	20 39	−40·1 E.	2 5	29 32
364	29	S	·994	3 57	20 9	16 55	21 36	−52·3 E.	2 34	33 29
365	30	**S**	·997	3 58	20 9	18 17	22 37	−64·4 E.	3 3	37 25
366	31	M	·999	3 59	20 9	19 33	23 40	−76·6 E.	0 3 32	18 41 22

	d	h	m		d	h	m
New Moon	**2**	**22**	**6**	**First Quarter**	**9**	**18**	**46**
Full Moon	**17**	**22**	**41**	**Last Quarter**	**25**	**18**	**0**

Meteor Notes.

The principal December shower is that of the *Geminids*, R.A. 107°, Dec. +33°, max. Dec. 10–11. The *Taurids* II., R.A. 80°, Dec. +23°, max. Dec. 6, also yield a well-marked shower. Fireball dates:—Dec. 2, 8, 11, 14, 19, 21, and 27.

Duration.	R.A.	Dec.	Remarks.	Duration.	R.A.	Dec.	Remarks.
	°	°			°	°	
Dec. 1–10 ……	44	+56	V. slow; faint.	Dec. 23–27……	130	+49	Near λ Ursæ Maj.
Dec. 1–12 ……	80	+23	Slow; bright.	Dec. 10–11……	143	+39	Swift; streaks.
Dec. 30–31……	92	+57	Slow; brilliant.	Dec. 7–10 ……	145	+ 7	Swift; streaks.
Dec. 4–12 ……	107	+33	Swift; short.	Dec. 1–16 ……	158	+72	Near λ Draconis.
Dec. 1–12 ……	110	+24	Rather swift.	Dec. 4–9………	161	+58	V. swift; streaks.
December ……	116	+32	Rather swift.	Dec. 18–28……	194	+67	Swift; streaks.

MERCURY (☿).

1888.		R.A.			Dec.			Diam.	Rises.		Transits.		Sets.	
		h	m	s	°	′		″	h	m	h	m	h	m
January	1	18	1	51	24	19	S.	4·8	19	37	23	22	3	5
	11	19	11	7	24	10		4·6	20	5	23	53	3	36
	21	20	22	6	21	35		4·7	20	17	0	21	4	26
	31	21	32	29	16	27		5·1	20	14	0	52	5	29
February	10	22	36	30	9	13		6·0	19	57	1	17	6	34
	20	23	14	55	2	41		7·8	19	20	1	15	7	5
March	1	23	2	21	2	10		10·4	18	26	0	23	6	15
	11	22	30	49	6	43		10·5	17	39	23	7	4	43
	21	22	31	34	9	12		8·9	17	15	22	32	3	51
	31	23	1	33	8	4		7·5	17	1	22	25	3	47
April	10	23	47	23	4	7	S.	6·4	16	47	22	32	4	13
	20	0	43	15	1	58	N.	5·7	16	33	22	49	5	1
	30	1	49	21	9	35		5·2	16	21	23	17	6	6
May	10	3	8	34	17	40		5·0	16	34	23	58	7	33
	20	4	37	5	23	42		5·3	16	26	0	43	9	2
	30	5	57	52	25	38		6·3	16	55	1	24	9	48
June	9	6	58	0	24	16		7·7	17	25	1	45	10	8
	19	7	31	47	21	22		9·6	17	37	1	39	9	41
	29	7	34	40	18	39		11·2	17	13	1	2	8	47
July	9	7	12	1	17	35		11·6	16	16	0	1	7	40
	19	6	54	52	18	37		10·0	15	14	23	0	6	49
	29	7	13	52	20	26		7·6	14	47	22	44	6	40
August	8	8	12	43	20	24		5·9	15	11	23	6	6	59
	18	9	32	24	16	21		5·1	16	19	23	47	7	18
	28	10	47	54	9	22		4·8	17	35	0	19	7	10
September	7	11	52	37	1	36	N.	5·0	18	39	0	45	6	57
	17	12	49	24	5	49	S.	5·2	19	33	1	2	6	35
	27	13	40	39	12	18		5·8	20	19	1	14	6	13
October	7	14	25	30	17	22		6·5	20	51	1	19	5	50
	17	14	55	8	20	3		8·0	20	53	1	9	5	24
	27	14	45	28	17	58		9·8	19	41	0	20	4	47
November	6	14	5	18	11	21		9·0	17	48	22	55	4	6
	16	14	14	55	11	1		6·8	17	26	22	31	3	38
	26	15	2	51	15	31		5·6	18	2	22	41	3	21
December	6	16	2	55	20	13		5·0	18	51	23	2	3	14
	16	17	8	12	23	34		4·7	19	40	23	29	3	17
	26	18	17	5	24	57	S.	4·6	20	17	23	58	3	36

At greatest elongation:—$18^\circ\ 5'$ E., Feb. $16^d\ 0^h$; $27^\circ\ 47'$ W., Mar. $30^d\ 13^h$; $24^\circ\ 24'$ E., June $11^d\ 14^h$; $19^\circ\ 26'$ W., July $28^d\ 22^h$; $25^\circ\ 5'$ E., Oct. $7^d\ 18^h$; $19^c\ 25'$ W., Nov. $16^d\ 16^h$.

In superior conjunction:—Jan. $18^d\ 8^h$; May $10^d\ 12^h$; Aug. $23^d\ 13^h$; Dec. $28^d\ 7^h$.

In inferior conjunction:—Mar. $3^d\ 7^h$; July $8^d\ 17^h$; Oct. $31^d\ 12^h$.

Stationary:—Feb. $22^d\ 13^h$; Mar. $15^d\ 16^h$; June $24^d\ 21^h$; July $19^d\ 12^h$; Oct. $20^d\ 9^h$; Nov. $9^d\ 6^h$.

VENUS (♀).

1888.		R.A.			Dec.			Diam.	Rises.		Transits.		Sets.	
		h	m	s	°	′		″	h	m	h	m	h	m
January	1	15	37	44	16	40	S.	18·3	16	22	20	56	1	30
	11	16	25	24	19	12		17·1	16	45	21	4	1	23
	21	17	15	7	21	0		15·9	17	8	21	15	1	22
	31	18	6	21	21	54		14·9	17	25	21	27	1	28
February	10	18	58	19	21	46		14·1	17	35	21	39	1	41
	20	19	50	7	20	36		13·4	17	40	21	52	2	2
March	1	20	40	59	18	26		12·8	17	37	22	3	2	27
	11	21	30	25	15	23		12·2	17	28	22	13	2	55
	21	22	18	18	11	37		11·8	17	15	22	21	3	24
	31	23	4	46	7	19		11·3	17	0	22	28	3	54
April	10	23	50	16	2	41	S.	11·0	16	42	22	34	4	24
	20	0	35	20	2	6	N.	10·7	16	24	22	40	4	54
	30	1	20	36	6	51		10·4	16	6	22	46	5	24
May	10	2	6	42	11	22		10·2	15	49	22	53	5	55
	20	2	54	11	15	27		10·0	15	34	23	1	6	26
	30	3	43	23	18	55		9·8	15	23	23	11	6	57
June	9	4	34	26	21	33		9·7	15	17	23	22	7	25
	19	5	27	2	23	11		9·6	15	20	23	36	7	50
	29	6	20	33	23	43		9·6	15	31	23	50	8	7
July	9	7	14	5	23	5		9·6	15	50	0	3	8	16
	19	8	6	44	21	19		9·6	16	16	0	16	8	18
	29	8	57	49	18	31		9·6	16	46	0	28	8	11
August	8	9	47	2	14	52		9·7	17	18	0	37	7	58
	18	10	34	29	10	32		9·8	17	50	0	45	7	41
	28	11	20	30	5	44		10·0	18	21	0	52	7	24
September	7	12	5	42	0	40	N.	10·1	18	53	0	58	7	5
	17	12	50	44	4	27	S.	10·3	19	25	1	3	6	43
	27	13	36	18	9	27		10·6	19	56	1	9	6	24
October	7	14	23	6	14	6		10·9	20	29	1	17	6	7
	17	15	11	38	18	11		11·2	21	3	1	26	5	51
	27	16	2	9	21	30		11·7	21	35	1	37	5	42
November	6	16	54	33	23	50		12·0	22	3	1	50	5	39
	16	17	48	16	25	1		12·5	22	25	2	4	5	45
	26	18	42	17	24	59		13·1	22	39	2	19	6	0
December	6	19	35	31	23	42		13·7	22	43	2	33	6	23
	16	20	26	55	21	18		14·5	22	38	2	45	6	52
	26	21	15	51	17	54	S.	15·3	22	25	2	54	7	22

In superior conjunction:—July 11 7^h.

MARS (♂).

1888.		R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
		h m s	° ′	″	h m	h m	h m
January	1	12 50 45	3 9 S.	8·0	12 20	18 7	23 54
	11	13 7 3	4 44	8·6	12 4	17 43	23 22
	21	13 21 50	6 7	9·3	11 47	17 19	22 51
	31	13 34 42	7 17	10·2	11 26	16 52	22 18
February	10	13 45 9	8 11	11·1	11 1	16 23	21 45
	20	13 52 30	8 46	12·3	10 32	15 51	21 10
March	1	13 56 3	9 2	13·7	9 57	15 15	20 33
	11	13 55 6	8 55	15·1	9 16	14 34	19 52
	21	13 49 13	8 26	16·3	8 28	13 49	19 10
	31	13 38 45	7 35	17·5	7 34	12 59	18 24
April	10	13 25 0	6 31	18·2	6 35	12 6	17 37
	20	13 10 24	5 27	18·4	5 36	11 12	16 48
	30	12 57 42	4 36	17·8	4 40	10 20	16 0
May	10	12 48 51	4 9	17·0	3 50	9 32	15 14
	20	12 44 53	4 11	15·9	3 7	8 49	14 31
	30	12 45 50	4 42	14·8	2 32	8 11	13 50
June	9	12 51 10	5 37	13·6	2 2	7 37	13 12
	19	13 0 19	6 52	12·6	1 39	7 7	12 35
	29	13 12 40	8 23	11·8	1 19	6 40	12 1
July	9	13 27 43	10 6	11·1	1 4	6 16	11 28
	19	13 45 8	11 57	10·4	0 53	5 54	10 55
	29	14 4 39	13 53	9·8	0 43	5 34	10 25
August	8	14 26 3	15 49	9·4	0 36	5 16	9 56
	18	14 49 17	17 43	9·0	0 32	5 0	9 28
	28	15 14 11	19 31	8·5	0 27	4 45	9 3
September	7	15 40 43	21 10	8·2	0 25	4 32	8 39
	17	16 8 48	22 35	7·9	0 23	4 21	8 19
	27	16 38 16	23 44	7·6	0 21	4 11	8 1
October	7	17 8 59	24 32	7·4	0 19	4 3	7 47
	17	17 40 43	24 58	7·1	0 14	3 55	7 36
	27	18 13 10	24 59	6·8	0 7	3 48	7 29
November	6	18 46 4	24 34	6·6	23 57	3 41	7 21
	16	19 19 4	23 42	6·4	23 44	3 35	7 25
	26	19 51 53	22 25	6·2	23 29	3 28	7 27
December	6	20 24 18	20 42	6·1	23 11	3 21	7 31
	16	20 56 7	18 38	6·0	22 50	3 14	7 35
	26	21 27 13	16 14 S.	5·8	22 27	3 5	7 43

In quadrature:—January 4^d 13^h; July 22^d 7^h.
Stationary:—March 4^d 2^h; May 22^d 23^h.
In opposition:—April 10^d 18^h.

JUPITER (♃).

1888.		R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
		h m s	° ′	″	h m	h m	h m
January	1	15 42 13	18 47 S.	30·0	16 35	20 57	1 22
	21	15 56 41	19 31	31·6	15 35	19 53	0 14
February	10	16 8 14	20 2	33·3	14 30	18 45	23 0
March	1	16 15 52	20 20	35·3	13 21	17 34	21 47
	21	16 18 41	20 25	37·7	12 6	16 18	20 30
April	10	16 16 19	20 18	39·9	10 44	14 57	19 10
	30	16 9 16	19 59	41·5	9 16	13 31	17 46
May	20	15 59 16	19 33	42·4	7 45	12 3	16 21
June	9	15 49 1	19 4	42·0	6 13	10 34	14 55
	29	15 41 13	18 43	40·7	4 45	9 8	13 31
July	19	15 37 40	18 36	38·7	3 22	7 45	12 8
August	8	15 39 3	18 46	36·5	2 6	6 28	10 50
	28	15 45 11	19 11	34·5	0 56	5 16	9 36
September	17	15 55 28	19 46	32·7	23 48	4 8	8 24
October	7	16 9 9	20 28 S.	32·1	22 47	3 2	7 14

SATURN (♄).

January	1	8 29 7	19 31 N.	18·2	5 53	13 44	21 35
	21	8 22 43	19 56	18·3	4 25	12 19	20 13
February	10	8 16 7	20 19	18·2	2 58	10 54	18 50
March	1	8 10 48	20 37	17·9	1 32	9 30	17 28
	21	8 7 54	20 47	17·4	0 10	8 9	16 8
April	10	8 7 58	20 48	16·7	22 47	6 50	14 49
	30	8 11 0	20 39	16·1	21 33	5 35	13 33
May	20	8 16 41	20 22	15·7	20 22	4 22	12 18
August	28	9 4 30	17 29	14·8	14 55	22 33	6 14
September	17	9 13 52	16 50	15·0	13 49	21 23	5 1
October	7	9 21 52	16 17	15·4	12 42	20 13	3 47
	27	9 27 54	15 52	15·9	11 32	19 0	2 32
November	16	9 31 28	15 39	16·6	10 18	17 45	1 16
December	6	9 32 12	15 39	17·2	9 0	16 27	23 54
	26	9 30 2	15 54 N.	17·7	7 38	15 6	22 34

Jupiter:—

In quadrature, Feb. $23^d\ 22^h$; Aug. $19^d\ 19^h$.
Stationary, Mar. $21^d\ 20^h$; July $23^d\ 10^h$.
In opposition, May $21^d\ 17^h$.
In conjunction, Dec. $8^d\ 11^h$.

Saturn:—

In opposition, Jan. $23^d\ 2^h$.
Stationary, Mar. $30^d\ 14^h$; Nov. $30^d\ 23^h$.
In quadrature, April $19^d\ 2^h$; Nov. $11^d\ 12^h$.
In conjunction Aug. $1^d\ 13^h$.

URANUS (♅).

1888.		R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
		h m s	° ′	″	h m	h m	h m
January	1	13 3 54	6 5 S.	3·8	12 46	18 19	23 52
	31	13 4 28	6 8	3·9	10 49	16 21	21 53
March	1	13 2 7	5 53	4·0	8 47	14 21	19 55
	31	12 57 47	5 25	4·0	6 43	12 18	17 53
April	30	12 53 11	4 57	3·9	4 38	10 16	15 54
May	30	12 50 2	4 38	3·9	2 34	8 15	13 56
June	29	12 49 27	4 36	3·8	0 36	6 16	11 56
November	26	13 16 13	7 24	3·6	15 23	20 49	2 19
December	26	13 20 39	7 50 S.	3·7	13 32	18 56	0 24

NEPTUNE (♆).

January	1	3 43 5	17 57 N.	1·3	1 18	8 59	16 40
	31	3 41 37	17 54	1·3	23 16	7 0	14 40
March	1	3 42 12	17 58	1·3	21 18	5 2	12 43
	31	3 44 45	18 8	1·3	19 22	3 7	10 49
April	30	3 48 43	18 22	1·3	17 26	1 13	8 56
July	29	4 0 50	18 57	1·3	11 40	19 27	3 18
August	28	4 2 21	18 59	1·3	9 44	17 31	1 22
September	27	4 1 50	18 56	1·3	7 45	15 32	23 19
October	27	3 59 30	18 49	1·3	5 46	13 32	21 18
November	26	3 56 8	18 38	1·3	3 46	11 31	19 16
December	26	3 52 55	18 30 N.	1·3	1 46	9 30	17 14

Uranus.—In quadrature, Jan. 7^{d} 12^{h}; July 4^{d} 8^{h}. Stationary, Jan. 21^{d} 13^{h}; June 20^{d} 3^{h}. In opposition, April 4^{d} 1^{h}. In conjunction, Oct. 10^{d} 1^{h}.

Neptune.—Stationary, Feb. 6^{d} 18^{h}; Sep. 4^{d} 8^{h}. In quadrature, Feb. 15^{d} 23^{h}; Aug. 24^{d} 16^{h}. In conjunction, May 19^{d} 22^{h}. In opposition, Nov. 22^{d} 6^{h}.

Minor Planets.

CERES ①.

	R.A.	Dec.	Transits.
1887.	h m s	° ′	h m
Dec. 29 ...	1 1 0	2 55 S.	6 30
1888.			
Jan. 22 ...	1 17 8	0 35 N.	5 12
Nov. 29 ...	8 40 1	24 56 N.	16 2
Dec. 23 ...	8 37 5	27 26 N.	14 25

JUNO ③.

	R.A.	Dec.	Transits.
1887.	h m s	° ′	h m
Dec. 29 ...	1 43 1	5 30 S.	7 12
1888.			
Jan. 22 ...	2 12 3	1 29	6 6
Nov. 29 ...	11 53 6	2 1	19 16
Dec. 23 ...	12 17 1	3 35 S.	18 5

PALLAS ②.

	R.A.	Dec.	Transits.
1888.			
Oct. 12 ...	4 56 8	20 7 S.	15 28
Nov. 5 ...	4 54 5	27 27	13 51
29 ...	4 37 5	32 3	12 0
Dec. 23 ...	4 18 1	31 57 S.	10 6

VESTA ④.

	R.A.	Dec.	Transits.
1888.			
Sept. 18 ...	0 55 9	6 51 S.	13 2
Oct. 12 ...	0 34 2	9 10	11 6
Nov. 5 ...	0 18 7	9 35	9 16
29 ...	0 17 8	7 59	7 41
Dec. 23 ...	0 30 6	5 3 S.	6 20

In opposition:—Pallas, Nov. 23; *Vesta,* Sept. 29.

ECLIPSES IN 1888.

In the year 1888 there will be three eclipses of the Sun and two of the Moon.

I. *A Total Eclipse of the Moon, January* 28.

Visible at Greenwich, and generally throughout North and South America, Europe, Asia, and Africa.

	d	h	m
First contact with Penumbra..................	Jan. 28	8	27·7
„ „ „ Shadow......................		9	30·4
Second „ „ „		10	30·9
Middle of Eclipse		11	20·1
Third contact with Shadow		12	9·2
Last „ „ „		13	9·5
„ „ „ Penumbra	28	14	11·9

Magnitude of Eclipse (Moon's diameter = 1) 1·647

The first contact with the Shadow takes place at 93° towards the E. of the N. point; the last at 74° towards the W. (for *Direct* image).

II. *A Partial Eclipse of the Sun, February* 11.

Visible only in the immediate neighbourhood of Cape Horn, and there only under very unfavourable conditions.

III. *A Partial Eclipse of the Sun, July* 8.

The northern limit just touches a corner of Australia, but the eclipse is practically not observable anywhere.

IV. *A Total Eclipse of the Moon, July* 22.

Partly visible at Greenwich, and generally throughout North and South America.

	d	h	m
First contact with Penumbra..................	July 22	14	55·5
„ „ „ Shadow......................		15	54·7
Second „ „ „		16	53·6
Middle of Eclipse		17	44·8
Third contact with Shadow		18	36·0
Last „ „ „		19	34·8
„ „ „ Penumbra		20	33·9

Magnitude of Eclipse (Moon's diameter = 1) 1·825

The first contact with the Shadow takes place at 82° towards the E. of the N. point; the last at 96° towards the W. (for *Direct* image).

V. *A Partial Eclipse of the Sun, August* 7.

(Just visible at Greenwich.)

Elements.

Greenwich M.T. of ☌ in R.A., Aug. 7d 5h 32m 35s·0.

☉'s and ☾'s R.A. 9h 12m 7s·04. Hourly motions 9s·54 and 143s·13.

	°	′	″			′	″
☉'s Declination	16	10	47·4	N.	Hourly motion	0	42·6
☾'s Declination	17	34	34·7	N.	Hourly motion	6	59·3
☉'s Equat. Hor. Parallax			8·7		☉'s true Semidiameter .	15	46·7
☾'s Equat. Hor. Parallax		57	13·1		☾'s true Semidiameter .	15	34·7

		d	h	m		°	′			°	′
Eclipse begins......	Aug.	7	5	2·4	in Long.	137	39·6	E.,	Lat.	71	7·6
Greatest eclipse ...		7	6	5·8		53	11·2	E.,		70	6·8
Eclipse ends		7	7	9·4		6	52·3	E.,		53	17·8

137°

Magnitude of greatest eclipse (Sun's diameter = 1) = 0·198.

The regions within which this eclipse is visible are chiefly the Arctic Ocean, Norway and Sweden, portions of Denmark and Greenland, and the extreme northerly parts of North America and Asia.

OCCULTATIONS, 1888.

(VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
Jan.			h m	°	h m	°
4	*b* Virginis	6	14 16	98	15 24	312
8	η Libræ	6	18 18	179	18 46	225
25	χ^3 Orionis	6	12 36	38	13 20	322
29	7 Leonis........	6½	5 41	172	5 55	205
29	ψ Leonis........	6	9 13	182	9 24	202
Feb.						
2	80 Virginis......	6	15 33	130	16 44	285
24	d^1 Cancri	6	9 30	59	10 28	322
25	7 Leonis........	6½	17 16	107	18 7	286
Mar.						
3	49 Libræ	5½	12 0‡	169	12 30	231
5	B.A.C. 6098 ...	6	14 28‡	72	15 25	310
18	B.A.C. 1351	6½	6 42	47	7 49	289
20	χ^3 Orionis	6	5 11	95	6 32	252
20	68 Orionis	6	10 21	85	11 25	279
22	B.A.C. 2683	6	12 23	67	13 13	317
28	80 Virginis......	6	7 49	132	8 43	275
30	η Libræ	6	13 20	102	14 30	301
Apr.						
16	χ^2 Orionis	6	9 27	82	10 26	279
19	θ Cancri........	6	7 34	126	8 45	262
22	B.A.C. 3837	6	6 9	58	7 32	347
27	χ Ophiuchi......	6	17 46	116	18 44‡	261
30	50 Sagittarii	6	15 19	168	15 33	189
May						
2	31 Capricorni....	6½	14 50	37	15 43	302
16	d^1 Cancri	6	11 5	145	11 45	240
20	*b* Virginis	6	12 57	79	13 44	325
24	η Libræ	6	10 52	47	11 23	356
24	θ Libræ	4½	15 45	129	16 39‡	255
25	B.A.C. 5700	6½	16 14	36	16 44	338
27	31 Sagittarii	6	10 17‡	98	11 23	274
29	20 Capricorni....	6	14 45	124	15 38	210
June						
24	50 Sagittarii	6	10 6	89	11 16	273
27	50 Aquarii......	6	14 28	3	14 59	316

Date.	Star.	Mag.	Disap.	Angle.	Reap.	Angle.
July			h m	°	h m	°
17	ξ^1 Libræ	6	7 22	38	7 40	370
18	θ Libræ	4½	9 2	183	9 23	215
19	B.A.C. 5700	6½	10 26	52	11 14	329
21	*o* Sagittarii......	4	12 42	23	13 15	327
23	20 Capricorni....	6	9 35	88	10 46	253
25	74 Aquarii......	6	12 57	84	14 11	229
31	*f* Tauri	4	11 44	117	12 22	205
Aug.						
1	B.A.C. 1351	6½	14 20	27	15 6	295
3	χ^3 Orionis	6	13 52	158	14 4	183
3	χ^4 Orionis	5	13 58	34	14 36	306
20	γ Capricorni	3½	12 58	68	14 10	250
22	ψ^3 Aquarii	5	9 46	121	10 30	195
26	ξ^2 Ceti..........	4	11 20	44	12 22	267
Sept.						
1	61 Geminorum ..	6	12 4‡	66	12 50	290
14	50 Sagittarii	6	10 58	74	12 4‡	267
16	30 Capricorni....	6	9 47	55	11 1	268
28	ζ^2 Geminorum ..	4	10 20‡	93	11 11	258
Oct.						
1	SATURN	..	3 59	106	4 49‡	284
2	*l* Leonis	5	15 0	88	15 52	302
11	B.A.C. 6524	6½	8 35	134	9 17	211
12	B.A.C. 6389	6	7 53	83	9 6	252
13	20 Capricorni....	6	7 5	24	7 55	305
15	74 Aquarii......	6	12 57	146	13 8	164
20	μ Ceti..........	4	11 39	103	12 41	203
22	δ^2 Tauri	6	14 53	35	15 59	292
24	χ^1 Orionis	4½	9 2	102	9 56	234
27	B.A.C. 2854	6	9 59‡	101	10 51	264
Nov.						
6	B.A.C. 5954	6	6 13	136	6 57‡	228
12	ψ^3 Aquarii	5	5 9	52	6 24	259
19	*i* Tauri	5½	9 57	351	10 9	331
20	B.A.C. 1835	6½	13 45	33	14 41	311
21	15 Geminorum ..	6	7 15	157	7 29	186
Dec.						
17	B.A.C. 1651	6½	5 42	33	6 26	297
17	B.A.C. 1733	6½	13 17	45	14 19	302

‡ Star below the horizon.

The angles are reckoned from the true N. point towards the right of the Moon's inverted image, in the direction N., E., S., W.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°	Sept.			h m	°
19	26 Ceti	6½	7 46	333	2	B.A.C. 2854	6	16 54	4
25	68 Orionis ...	6	16 57	2	19	B.A.C. 8274	6	8 56	337
27	B.A.C. 2683	6	17 10	12	Oct.				
May					16	B.A.C. 8214	6½	9 21	333
15	61 Geminor.	6	9 48	9	Nov.				
20	B.A.C. 3996	6	7 0	27	25	37 Leonis ...	0	19 25	24
July					29	B.A.C. 4572		17 59	28
18	49 Libræ ...	5½	12 20	12	Dec.				
Aug.					16	B.A.C. 1468	6	15 31	353
1	63 Tauri ...	6	14 44	341	16	*i* Tauri	5½	18 1	354
21	50 Aquarii ...	6	8 17	345					
22	ψ^2 Aquarii ...	4½	9 55	339					

South Near Approaches.

Jan.			h m	°	Apr.			h m	°
22	*f* Tauri	4	13 19	168	24	65 Virginis	6	16 19	201
Feb.					Nov.				
20	*α* Tauri	1	3 56	162	16	ξ^2 Ceti	4	9 11	151
21	119 Tauri ...	5½	7 10	170	23	B.A.C. 2854	6	18 53	195
Mar.					27	B.A.C. 3996	6	13 44	200
18	75 Tauri ...	6	10 2	173					

For Satellites of MARS, see page 55.

PHENOMENA OF JUPITER'S SATELLITES, 1888.

January.

d	h	m	
1	6	26	i. Ec. D.
	9	28	i. Oc. R.
2	2	57	ii. Ec. D.
	4	31	i. Tr. I.
	5	18	iii. Tr. I.
	6	44	i. Tr. E.
	7	6	ii. Oc. R.
	7	7	iii. Tr. E.
3	0	55	i. Ec. D.
	3	58	i. Oc. R.
	23	1	i. Tr. I.
	23	13	ii. Tr. I.
4	1	13	i. Tr. E.
	1	44	ii. Tr. E.
	19	23	i. Ec. D.
	22	27	i. Oc. R.

January (con.).

d	h	m	
5	16	7	iii. Ec. D.
	16	14	ii. Ec. D.
	17	30	i. Tr. I.
	17	38	iii. Ec. R.
	19	39	iii. Oc. D.
	19	43	i. Tr. E.
	20	28	ii. Oc. R.
	21	27	iii. Oc. R.
6	13	52	i. Ec. D.
	16	57	i. Oc. R.
7	12	0	i. Tr. I.
	12	37	ii. Tr. I.
	14	12	i. Tr. E.
	15	7	ii. Tr. E.
8	8	20	i. Ec. D.
	11	27	i. Oc. R.

January (con.).

d	h	m	
9	5	30	ii. Ec. D.
	6	29	i. Tr. I.
	8	42	i. Tr. E.
	9	38	iii. Tr. I.
	9	49	ii. Oc. R.
	11	25	iii. Tr. E.
10	2	48	i. Ec. D.
	5	56	i. Oc. R.
11	0	59	i. Tr. I.
	2	0	ii. Tr. I.
	3	11	i. Tr. E.
	4	30	ii. Tr. E.
	21	17	i. Ec. D.
12	0	26	i. Oc. R.
	18	47	ii. Ec. D.
	19	28	i. Tr. I.

January (con.).

d	h	m	
12	20	5	iii. Ec. D.
	21	36	iii. Ec. R.
	21	40	i. Tr. E.
	23	11	ii. Oc. R.
	23	58	iii. Oc. D.
13	1	44	iii. Oc. R.
	15	45	i. Ec. D.
	18	56	i. Oc. R.
14	13	58	i. Tr. I.
	15	23	ii. Tr. I.
	16	10	i. Tr. E.
	17	53	ii. Tr. E.
15	10	14	i. Ec. D.
	13	25	i. Oc. R.
16	8	3	ii. Ec. D.
	8	27	i. Tr. I.

January (con.).

d	h	m	
16	10	39	i. Tr. E.
	12	32	ii. Oc. R.
	13	55	iii. Tr. I.
	15	41	iii. Tr. E.
17	4	42	i. Ec. D.
	7	55	i. Oc. R.
18	2	57	i. Tr. I.
	4	45	ii. Tr. I.
	5	9	i. Tr. E.
	7	15	ii. Tr. E.
	23	11	i. Ec. D.
19	2	24	i. Oc. R.
	21	20	ii. Ec. D.
	21	26	i. Tr. I.
	23	38	i. Tr. E.
20	0	3	iii. Ec. D.
	1	34	iii. Ec. R.
	1	52	ii. Oc. R.
	4	15	iii. Oc. D.
	6	0	iii. Oc. R.
	17	39	i. Ec. D.
	20	54	i. Oc. R.
21	15	56	i. Tr. I.
	18	8	ii. Tr. I.
	18	8	i. Tr. E.
	20	37	ii. Tr. E.
22	12	7	i. Ec. D.
	15	23	i. Oc. R.
23	10	24	i. Tr. I.
	10	36	ii. Ec. D.
	12	36	i. Tr. E.
	15	13	ii. Oc. R.
	18	10	iii. Tr. I.
	19	54	iii. Tr. E.
24	6	36	i. Ec. D.
	9	52	i. Oc. R.
25	4	54	i. Tr. I.
	7	6	i. Tr. E.
	7	29	ii. Tr. I.
	9	58	ii. Tr. E.
26	1	4	i. Ec. D.
	4	22	i. Oc. R.
	23	23	i. Tr. I.
	23	53	ii. Ec. D.
27	1	35	i. Tr. E.
	4	0	iii. Ec. D.
	4	33	ii. Oc. R.
	5	31	iii. Ec. R.
	8	29	iii. Oc. D.
	10	12	iii. Oc. R.
	19	32	i. Ec. D.
	22	51	i. Oc. R.
28	17	52	i. Tr. I.
	20	4	i. Tr. E.
	20	51	ii. Tr. I.
	23	20	ii. Tr. E.
29	14	1	i. Ec. D.
	17	20	i. Oc. R.

January (con.).

d	h	m	
30	12	21	i. Tr. I.
	13	9	ii. Ec. D.
	14	33	i. Tr. E.
	17	52	ii. Oc. R.
	22	23	iii. Tr. I.
31	0	5	iii. Tr. E.
	8	29	i. Ec. D.
	11	49	i. Oc. R.

February.

d	h	m	
1	6	50	i. Tr. I.
	9	2	i. Tr. E.
	10	12	ii. Tr. I.
	12	40	ii. Tr. E.
2	2	58	i. Ec. D.
	6	18	i. Oc. R.
3	1	19	i. Tr. I.
	2	25	ii. Ec. D.
	3	31	i. Tr. E.
	7	12	ii. Oc. R.
	7	58	iii. Ec. D.
	9	29	iii. Ec. R.
	12	40	iii. Oc. D.
	14	21	iii. Oc. R.
	21	26	i. Ec. D.
4	0	48	i. Oc. R.
	19	48	i. Tr. I.
	22	0	i. Tr. E.
	23	33	ii. Tr. I.
5	2	1	ii. Tr. E.
	15	54	i. Ec. D.
	19	17	i. Oc. R.
6	14	16	i. Tr. I.
	15	42	ii. Ec. D.
	16	28	i. Tr. E.
	20	30	ii. Oc. R.
7	2	32	iii. Tr. I.
	4	13	iii. Tr. E.
	10	23	i. Ec. D.
	13	46	i. Oc. R.
8	8	45	i. Tr. I.
	10	57	i. Tr. E.
	12	53	ii. Tr. I.
	15	21	ii. Tr. E.
9	4	51	i. Ec. D.
	8	14	i. Oc. R.
10	3	14	i. Tr. I.
	4	58	ii. Ec. D.
	5	26	i. Tr. E.
	9	49	ii. Oc. R.
	11	55	iii. Ec. D.
	13	27	iii. Ec. R.
	16	48	iii. Oc. D.
	18	27	iii. Oc. R.
	23	19	i. Ec. D.
11	2	43	i. Oc. R.
	21	43	i. Tr. I.

February (con.).

d	h	m	
11	23	55	i. Tr. E.
12	2	13	ii. Tr. I.
	4	41	ii. Tr. E.
	17	48	i. Ec. D.
	21	12	i. Oc. R.
13	16	11	i. Tr. I.
	18	15	ii. Ec. D.
	18	23	i. Tr. E.
	20	39	ii. Ec. R.
	20	39	ii. Oc. D.
	23	7	ii. Oc. R.
14	6	38	iii. Tr. I.
	8	17	iii. Tr. E.
	12	16	i. Ec. D.
	15	41	i. Oc. R.
15	10	40	i. Tr. I.
	12	52	i. Tr. E.
	15	32	ii. Tr. I.
	18	0	ii. Tr. E.
16	6	45	i. Ec. D.
	10	9	i. Oc. R.
17	5	8	i. Tr. I.
	7	20	i. Tr. E.
	7	31	ii. Ec. D.
	9	55	ii. Ec. R.
	9	57	ii. Oc. D.
	12	25	ii. Oc. R.
	15	53	iii. Ec. D.
	17	25	iii. Ec. R.
	20	52	iii. Oc. D.
	22	30	iii. Oc. R.
18	1	13	i. Ec. D.
	4	38	i. Oc. R.
	23	37	i. Tr. I.
19	1	49	i. Tr. E.
	4	52	ii. Tr. I.
	7	19	ii. Tr. E.
	19	41	i. Ec. D.
	23	7	i. Oc. R.
20	18	5	i. Tr. I.
	20	17	i. Tr. E.
	20	48	ii. Ec. D.
	23	12	ii. Ec. R.
	23	15	ii. Oc. D.
21	1	42	ii. Oc. R.
	10	40	iii. Tr. I.
	12	17	iii. Tr. E.
	14	10	i. Ec. D.
	17	35	i. Oc. R.
22	12	34	i. Tr. I.
	14	46	i. Tr. E.
	18	10	ii. Tr. I.
	20	37	ii. Tr. E.
23	8	38	i. Ec. D.
	12	3	i. Oc. R.
24	7	2	i. Tr. I.
	9	13	i. Tr. E.
	10	4	ii. Ec. D.

February (con.).

d	h	m	
24	12	28	ii. Ec. R.
	12	32	ii. Oc. D.
	14	59	ii. Oc. R.
	19	51	iii. Ec. D.
	21	23	iii. Ec. R.
25	0	53	iii. Oc. D.
	2	29	iii. Oc. R.
	3	6	i. Ec. D.
	6	32	i. Oc. R.
26	1	30	i. Tr. I.
	3	42	i. Tr. E.
	7	28	ii. Tr. I.
	9	55	ii. Tr. E.
	21	35	i. Ec. D.
27	1	0	i. Oc. R.
	19	58	i. Tr. I.
	22	10	i. Tr. E.
	23	21	ii. Ec. D.
28	1	45	ii. Ec. R.
	1	48	ii. Oc. D.
	4	15	ii. Oc. R.
	14	38	iii. Tr. I.
	16	3	i. Ec. D.
	16	13	iii. Tr. E.
	19	28	i. Oc. R.
29	14	26	i. Tr. I.
	16	38	i. Tr. E.
	20	45	ii. Tr. I.
	23	12	ii. Tr. E.

March.

d	h	m	
1	10	32	i. Ec. D.
	13	57	i. Oc. R.
2	8	55	i. Tr. I.
	11	6	i. Tr. E.
	12	37	ii. Ec. D.
	15	1	ii. Ec. R.
	15	4	ii. Oc. D.
	17	31	ii. Oc. R.
	23	48	iii. Ec. D.
3	1	21	iii. Ec. R.
	4	50	iii. Oc. D.
	5	0	i. Ec. D.
	6	24	iii. Oc. R.
	8	25	i. Oc. R.
4	3	23	i. Tr. I.
	5	34	i. Tr. E.
	10	3	ii. Tr. I.
	12	29	ii. Tr. E.
	23	28	i. Ec. D.
5	2	53	i. Oc. R.
	21	50	i. Tr. I.
6	0	2	i. Tr. E.
	1	54	ii. Ec. D.
	4	18	ii. Ec. R.
	4	20	ii. Oc. D.
	6	46	ii. Oc. R.

March (con.).

d	h	m	
6	17	57	i. Ec. D.
	18	32	iii. Tr. I.
	20	5	iii. Tr. E.
	21	21	i. Oc. R.
7	16	18	i. Tr. I.
	18	30	i. Tr. E.
	23	18	ii. Tr. I.
8	1	45	ii. Tr. E.
	12	25	i. Ec. D.
	15	49	i. Oc. R.
9	10	46	i. Tr. I.
	12	58	i. Tr. E.
	15	11	ii. Ec. D.
	20	1	ii. Oc. R.
10	3	45	iii. Ec. D.
	5	19	iii. Ec. R.
	6	53	i. Ec. D.
	8	42	iii. Oc. D.
	10	14	iii. Oc. R.
	10	16	i. Oc. R.
11	5	14	i. Tr. I.
	7	25	i. Tr. E.
	12	35	ii. Tr. I.
	15	1	ii. Tr. E.
12	1	22	i. Ec. D.
	4	44	i. Oc. R.
	23	41	i. Tr. I.
13	1	53	i. Tr. E.
	4	27	ii. Ec. D.
	9	16	ii. Oc. R.
	19	50	i. Ec. D.
	22	22	iii. Tr. I.
	23	12	i. Oc. R.
	23	53	iii. Tr. E.
14	18	9	i. Tr. I.
	20	21	i. Tr. E.
15	1	50	ii. Tr. I.
	4	16	ii. Tr. E.
	14	18	i. Ec. D.
	17	40	i. Oc. R.
16	12	37	i. Tr. I.
	14	48	i. Tr. E.
	17	44	ii. Ec. D.
	22	30	ii. Oc. R.
17	7	43	iii. Ec. D.
	8	47	i. Ec. D.
	9	17	iii. Ec. R.
	12	7	i. Oc. R.
	12	29	iii. Oc. D.
	14	0	iii. Oc. R.
18	7	4	i. Tr. I.
	9	16	i. Tr. E.
	15	5	ii. Tr. I.
	17	31	ii. Tr. E.
19	3	15	i. Ec. D.
	6	35	i. Oc. R.
20	1	32	i. Tr. I.
	3	43	i. Tr. E.

March (con.).

d	h	m	
20	7	1	ii. Ec. D.
	11	43	ii. Oc. R.
	21	43	i. Ec. D.
21	1	2	i. Oc. R.
	2	7	iii. Tr. I.
	3	38	iii. Tr. E.
	19	59	i. Tr. I.
	22	10	i. Tr. E.
22	4	18	ii. Tr. I.
	6	44	ii. Tr. E.
	16	12	i. Ec. D.
	19	30	i. Oc. R.
23	14	26	i. Tr. I.
	16	38	i. Tr. E.
	20	17	ii. Ec. D.
24	0	56	ii. Oc. R.
	10	40	i. Ec. D.
	11	40	iii. Ec. D.
	13	14	iii. Ec. R.
	13	57	i. Oc. R.
	16	12	iii. Oc. D.
	17	41	iii. Oc. R.
25	8	53	i. Tr. I.
	11	5	i. Tr. E.
	17	32	ii. Tr. I.
	19	58	ii. Tr. E.
26	5	9	i. Ec. D.
	8	24	i. Oc. R.
27	3	20	i. Tr. I.
	5	32	i. Tr. E.
	9	34	ii. Ec. D.
	14	8	ii. Oc. R.
	23	37	i. Ec. D.
28	2	51	i. Oc. R.
	5	47	iii. Tr. I.
	7	16	iii. Tr. E.
	21	48	i. Tr. I.
	23	59	i. Tr. E.
29	6	45	ii. Tr. I.
	9	10	ii. Tr. E.
	18	5	i. Ec. D.
	21	19	i. Oc. R.
30	16	15	i. Tr. I.
	18	26	i. Tr. E.
	22	51	ii. Ec. D.
31	3	20	ii. Oc. R.
	12	34	i. Ec. D.
	15	37	iii. Ec. D.
	15	45	i. Oc. R.
	17	12	iii. Ec. R.
	19	50	iii. Oc. D.
	21	18	iii. Oc. R.

April.

d	h	m	
1	10	42	i. Tr. I.
	12	53	i. Tr. E.
	19	57	ii. Tr. I.

April (con.).

d	h	m	
1	22	23	ii. Tr. E.
2	7	2	i. Ec. D.
	10	12	i. Oc. R.
3	5	9	i. Tr. I.
	7	20	i. Tr. E.
	12	8	ii. Ec. D.
	16	31	ii. Oc. R.
4	1	30	i. Ec. D.
	4	39	i. Oc. R.
	9	24	iii. Tr. I.
	10	52	iii. Tr. E.
	23	35	i. Tr. I.
5	1	47	i. Tr. E.
	9	9	ii. Tr. I.
	11	34	ii. Tr. E.
	19	59	i. Ec. D.
	23	6	i. Oc. R.
6	18	2	i. Tr. I.
	20	13	i. Tr. E.
7	1	25	ii. Ec. D.
	5	42	ii. Oc. R.
	14	27	i. Ec. D.
	17	33	i. Oc. R.
	19	35	iii. Ec. D.
	21	11	iii. Ec. R.
	23	24	iii. Oc. D.
8	0	51	iii. Oc. R.
	12	29	i. Tr. I.
	14	40	i. Tr. E.
	22	20	ii. Tr. I.
9	0	46	ii. Tr. E.
	8	56	i. Ec. D.
	12	0	i. Oc. R.
10	6	55	i. Tr. I.
	9	7	i. Tr. E.
	14	42	ii. Ec. D.
	18	53	ii. Oc. R.
11	3	24	i. Ec. D.
	6	26	i. Oc. R.
	12	54	iii. Tr. I.
	14	22	iii. Tr. E.
12	1	22	i. Tr. I.
	3	33	i. Tr. E.
	11	30	ii. Tr. I.
	13	56	ii. Tr. E.
	21	52	i. Ec. D.
13	0	53	i. Oc. R.
	19	48	i. Tr. I.
	22	0	i. Tr. E.
14	3	59	ii. Ec. D.
	8	3	ii. Oc. R.
	16	21	i. Ec. D.
	19	20	i. Oc. R.
	23	33	iii. Ec. D.
15	1	9	iii. Ec. R.
	2	54	iii. Oc. D.
	4	20	iii. Oc. R.
	14	15	i. Tr. I.

April (con.).

d	h	m	
15	16	27	i. Tr. E.
16	0	41	ii. Tr. I.
	3	6	ii. Tr. E.
	10	49	i. Ec. D.
	13	46	i. Oc. R.
17	8	42	i. Tr. I.
	10	53	i. Tr. E.
	17	16	ii. Ec. D.
	21	12	ii. Oc. R.
18	5	17	i. Ec. D.
	8	13	i. Oc. R.
	16	22	iii. Tr. I.
	17	48	iii. Tr. E.
19	3	8	i. Tr. I.
	5	20	i. Tr. E.
	13	50	ii. Tr. I.
	16	16	ii. Tr. E.
	23	46	i. Ec. D.
20	2	39	i. Oc. R.
	21	34	i. Tr. I.
	23	46	i. Tr. E.
21	6	34	ii. Ec. D.
	10	21	ii. Oc. R.
	18	14	i. Ec. D.
	21	5	i. Oc. R.
22	3	31	iii. Ec. D.
	5	8	iii. Ec. R.
	6	19	iii. Oc. D.
	7	46	iii. Oc. R.
	16	1	i. Tr. I.
	18	12	i. Tr. E.
23	3	0	ii. Tr. I.
	5	25	ii. Tr. E.
	12	43	i. Ec. D.
	15	32	i. Oc. R.
24	10	27	i. Tr. I.
	12	38	i. Tr. E.
	19	51	ii. Ec. D.
	23	30	ii. Oc. R.
25	7	11	i. Ec. D.
	9	58	i. Oc. R.
	19	45	iii. Tr. I.
	21	12	iii. Tr. E.
26	4	53	i. Tr. I.
	7	5	i. Tr. E.
	16	8	ii. Tr. I.
	18	33	ii. Tr. E.
27	1	40	i. Ec. D.
	4	24	i. Oc. R.
	23	19	i. Tr. I.
28	1	31	i. Tr. E.
	9	8	ii. Ec. D.
	12	39	ii. Oc. R.
	20	8	i. Ec. D.
	22	50	i. Oc. R.
29	7	28	iii. Ec. D.
	9	6	iii. Ec. R.
	9	41	iii. Oc. D.

April (con.).

d	h	m	
29	11	8	iii. Oc. R.
	17	45	i. Tr. I.
	19	57	i. Tr. E.
30	5	17	ii. Tr. I.
	7	42	ii. Tr. E.
	14	36	i. Ec. D.
	17	16	i. Oc. R.

May.

d	h	m	
1	12	12	i. Tr. I.
	14	23	i. Tr. E.
	22	25	ii. Ec. D.
2	1	47	ii. Oc. R.
	9	5	i. Ec. D.
	11	42	i. Oc. R.
	23	6	iii. Tr. I.
3	0	33	iii. Tr. E.
	6	38	i. Tr. I.
	8	49	i. Tr. E.
	18	24	ii. Tr. I.
	20	50	ii. Tr. E.
4	3	33	i. Ec. D.
	6	9	i. Oc. R.
5	1	4	i. Tr. I.
	3	15	i. Tr. E.
	11	43	ii. Ec. D.
	14	55	ii. Oc. R.
	22	2	i. Ec. D.
6	0	35	i. Oc. R.
	11	26	iii. Ec. D.
	14	27	iii. Oc. R.
	19	30	i. Tr. I.
	21	41	i. Tr. E.
7	7	32	ii. Tr. I.
	9	58	ii. Tr. E.
	16	30	i. Ec. D.
	19	1	i. Oc. R.
8	13	56	i. Tr. I.
	16	7	i. Tr. E.
9	1	0	ii. Ec. D.
	4	3	ii. Oc. R.
	10	59	i. Ec. D.
	13	27	i. Oc. R.
10	2	25	iii. Tr. I.
	3	53	iii. Tr. E.
	8	22	i. Tr. I.
	10	33	i. Tr. E.
	20	39	ii. Tr. I.
	23	5	ii. Tr. E.
11	5	27	i. Ec. D.
	7	53	i. Oc. R.
12	2	48	i. Tr. I.
	4	59	i. Tr. E.
	14	18	ii. Ec. D.
	17	10	ii. Oc. R.
	23	56	i. Ec. D.
13	2	19	i. Oc. R.

May (con.).

d	h	m	
13	15	23	iii. Ec. D.
	17	45	iii. Oc. R.
	21	14	i. Tr. I.
	23	25	i. Tr. E.
14	9	46	ii. Tr. I.
	12	12	ii. Tr. E.
	18	24	i. Ec. D.
	20	45	i. Oc. R.
15	15	40	i. Tr. I.
	17	51	i. Tr. E.
16	3	36	ii. Ec. D.
	6	18	ii. Oc. R.
	12	53	i. Ec. D.
	15	10	i. Oc. R.
17	5	41	iii. Tr. I.
	7	11	iii. Tr. E.
	10	6	i. Tr. I.
	12	17	i. Tr. E.
	22	53	ii. Tr. I.
18	1	19	ii. Tr. E.
	7	21	i. Ec. D.
	9	36	i. Oc. R.
19	4	32	i. Tr. I.
	6	43	i. Tr. E.
	16	54	ii. Ec. D.
	19	25	ii. Oc. R.
20	1	50	i. Ec. D.
	4	2	i. Oc. R.
	19	21	iii. Ec. D.
	21	3	iii. Oc. R.
	22	58	i. Tr. I.
21	1	9	i. Tr. E.
	12	0	ii. Tr. I.
	14	26	ii. Tr. E.
	20	17	i. Oc. D.
	22	28	i. Oc. R.
22	17	24	i. Tr. I.
	19	35	i. Tr. E.
23	6	6	ii. Oc. D.
	8	36	ii. Ec. R.
	14	42	i. Oc. D.
	16	55	i. Ec. R.
24	8	56	iii. Tr. I.
	10	29	iii. Tr. E.
	11	50	i. Tr. I.
	14	1	i. Tr. E.
25	1	7	ii. Tr. I.
	3	33	ii. Tr. E.
	9	8	i. Oc. D.
	11	23	i. Ec. R.
26	6	15	i. Tr. I.
	8	27	i. Tr. E.
	19	13	ii. Oc. D.
	21	54	ii. Ec. R.
27	3	34	i. Oc. D.
	5	52	i. Ec. R.
	22	47	iii. Oc. D.
28	0	42	i. Tr. I.

May (con.).

d	h	m	
28	1	1	iii. Ec. R.
	2	53	i. Tr. E.
	14	13	ii. Tr. I.
	16	40	ii. Tr. E.
	22	0	i. Oc. D.
29	0	20	i. Ec. R.
	19	8	i. Tr. I.
	21	19	i. Tr. E.
30	8	21	ii. Oc. D.
	11	12	ii. Ec. R.
	16	26	i. Oc. D.
	18	49	i. Ec. R.
31	12	12	iii. Tr. I.
	13	34	i. Tr. I.
	13	47	iii. Tr. E.
	15	45	i. Tr. E.

June.

d	h	m	
1	3	20	ii. Tr. I.
	5	47	ii. Tr. E.
	10	52	i. Oc. D.
	13	17	i. Ec. R.
2	8	0	i. Tr. I.
	10	12	i. Tr. E.
	21	29	ii. Oc. D.
3	0	30	ii. Ec. R.
	5	18	i. Oc. D.
	7	46	i. Ec. R.
4	2	3	iii. Oc. D.
	2	26	i. Tr. I.
	4	38	i. Tr. E.
	5	1	iii. Ec. R.
	16	27	ii. Tr. I.
	18	55	ii. Tr. E.
	23	44	i. Oc. D.
5	2	14	i. Ec. R.
	20	52	i. Tr. I.
	23	4	i. Tr. E.
6	10	36	ii. Oc. D.
	13	48	ii. Ec. R.
	18	11	i. Oc. D.
	20	43	i. Ec. R.
7	15	18	i. Tr. I.
	15	28	iii. Tr. I.
	17	7	iii. Tr. E.
	17	30	i. Tr. E.
8	5	35	ii. Tr. I.
	8	2	ii. Tr. E.
	12	37	i. Oc. D.
	15	11	i. Ec. R.
9	9	44	i. Tr. I.
	11	56	i. Tr. E.
	23	45	ii. Oc. D.
10	3	7	ii. Ec. R.
	7	3	i. Oc. D.
	9	40	i. Ec. R.
11	4	11	i. Tr. I.

June (con.).

d	h	m	
11	5	21	iii. Oc. D.
	6	23	i. Tr. E.
	7	1	iii. Oc. R.
	7	17	iii. Ec. D.
	9	0	iii. Ec. R.
	18	42	ii. Tr. I.
	21	10	ii. Tr. E.
12	1	29	i. Oc. D.
	4	9	i. Ec. R.
	22	37	i. Tr. I.
13	0	49	i. Tr. E.
	12	53	ii. Oc. D.
	16	24	ii. Ec. R.
	19	55	i. Oc. D.
	22	37	i. Ec. R.
14	17	3	i. Tr. I.
	18	48	iii. Tr. I.
	19	16	i. Tr. E.
	20	30	iii. Tr. E.
15	7	50	ii. Tr. I.
	10	18	ii. Tr. E.
	14	22	i. Oc. D.
	17	6	i. Ec. R.
16	11	30	i. Tr. I.
	13	42	i. Tr. E.
17	2	2	ii. Oc. D.
	5	43	ii. Ec. R.
	8	48	i. Oc. D.
	11	34	i. Ec. R.
18	5	56	i. Tr. I.
	8	8	i. Tr. E.
	8	41	iii. Oc. D.
	10	25	iii. Oc. R.
	11	15	iii. Ec. D.
	12	59	iii. Ec. R.
	20	58	ii. Tr. I.
	23	26	ii. Tr. E.
19	3	14	i. Oc. D.
	6	3	i. Ec. R.
20	0	23	i. Tr. I.
	2	35	i. Tr. E.
	15	11	ii. Oc. D.
	19	1	ii. Ec. R.
	21	41	i. Oc. D.
21	0	32	i. Ec. R.
	18	49	i. Tr. I.
	21	2	i. Tr. E.
	22	10	iii. Tr. I.
	23	55	iii. Tr. E.
22	10	6	ii. Tr. I.
	12	35	ii. Tr. E.
	16	7	i. Oc. D.
	19	0	i. Ec. R.
23	13	16	i. Tr. I.
	15	28	i. Tr. E.
24	4	21	ii. Oc. D.
	8	20	ii. Ec. R.
	10	34	i. Oc. D.

June (con.).

d	h	m	
24	13	29	i. Ec. R.
25	7	43	i. Tr. I.
	9	55	i. Tr. E.
	12	4	iii. Oc. D.
	13	51	iii. Oc. R.
	15	13	iii. Ec. D.
	16	58	iii. Ec. R.
	23	15	ii. Tr. I.
26	1	44	ii. Tr. E.
	5	0	i. Oc. D.
	7	57	i. Ec. R.
27	2	9	i. Tr. I.
	4	22	i. Tr. E.
	17	31	ii. Oc. D.
	21	38	ii. Ec. R.
	23	27	i. Oc. D.
28	2	26	i. Ec. R.
	20	36	i. Tr. I.
	22	49	i. Tr. E.
29	1	36	iii. Tr. I.
	3	25	iii. Tr. E.
	12	24	ii. Tr. I.
	14	54	ii. Tr. E.
	17	54	i. Oc. D.
	20	55	i. Ec. R.
30	15	3	i. Tr. I.
	17	16	i. Tr. E.

July.

d	h	m	
1	6	42	ii. Oc. D.
	10	57	ii. Ec. R.
	12	21	i. Oc. D.
	15	23	i. Ec. R.
2	9	30	i. Tr. I.
	11	43	i. Tr. E.
	15	31	iii. Oc. D.
	17	21	iii. Oc. R.
	19	12	iii. Ec. D.
	20	58	iii. Ec. R.
3	1	34	ii. Tr. I.
	4	4	ii. Tr. E.
	6	48	i. Oc. D.
	9	52	i. Ec. R.
4	3	57	i. Tr. I.
	6	10	i. Tr. E.
	19	53	ii. Oc. D.
5	0	15	ii. Ec. R.
	1	14	i. Oc. D.
	4	21	i. Ec. R.
	22	24	i. Tr. I.
6	0	37	i. Tr. E.
	5	5	iii. Tr. I.
	6	57	iii. Tr. E.
	14	44	ii. Tr. I.
	17	15	ii. Tr. E.
	19	41	i. Oc. D.
	22	49	i. Ec. R.

July (con.).

d	h	m	
7	16	52	i. Tr. I.
	19	4	i. Tr. E.
8	9	5	ii. Oc. D.
	13	34	ii. Ec. R.
	14	9	i. Oc. D.
	17	18	i. Ec. R.
9	11	19	i. Tr. I.
	13	31	i. Tr. E.
	19	2	iii. Oc. D.
	20	56	iii. Oc. R.
	23	10	iii. Ec. D.
10	0	57	iii. Ec. R.
	3	55	ii. Tr. I.
	6	26	ii. Tr. E.
	8	36	i. Oc. D.
	11	47	i. Ec. R.
11	5	46	i. Tr. I.
	7	59	i. Tr. E.
	22	17	ii. Oc. D.
12	2	52	ii. Ec. R.
	3	3	i. Oc. D.
	6	15	i. Ec. R.
13	0	13	i. Tr. I.
	2	26	i. Tr. E.
	8	39	iii. Tr. I.
	10	34	iii. Tr. E.
	17	7	ii. Tr. I.
	19	37	ii. Tr. E.
	21	30	i. Oc. D.
14	0	44	i. Ec. R.
	18	41	i. Tr. I.
	20	54	i. Tr. E.
15	11	30	ii. Oc. D.
	15	57	i. Oc. D.
	16	12	ii. Ec. R.
	19	12	i. Ec. R.
16	13	8	i. Tr. I.
	15	21	i. Tr. E.
	22	38	iii. Oc. D.
17	0	35	iii. Oc. R.
	3	9	iii. Ec. D.
	4	57	iii. Ec. R.
	6	19	ii. Tr. I.
	8	50	ii. Tr. E.
	10	25	i. Oc. D.
	13	41	i. Ec. R.
18	7	36	i. Tr. I.
	9	49	i. Tr. E.
19	0	44	ii. Oc. D.
	4	52	i. Oc. D.
	5	30	ii. Ec. R.
	8	10	i. Ec. R.
20	2	4	i. Tr. I.
	4	17	i. Tr. E.
	12	17	iii. Tr. I.
	14	15	iii. Tr. E.
	19	31	ii. Tr. I.
	22	2	ii. Tr. E.

July (con.).

d	h	m	
20	23	20	i. Oc. D.
21	2	39	i. Ec. R.
	20	31	i. Tr. I.
	22	44	i. Tr. E.
22	13	58	ii. Oc. D.
	17	47	i. Oc. D.
	18	49	ii. Ec. R.
	21	7	i. Ec. R.
23	14	59	i. Tr. I.
	17	12	i. Tr. E.
24	2	18	iii. Oc. D.
	4	18	iii. Oc. R.
	7	9	iii. Ec. D.
	8	44	ii. Tr. I.
	8	57	iii. Ec. R.
	11	16	ii. Tr. E.
	12	15	i. Oc. D.
	15	36	i. Ec. R.
25	9	27	i. Tr. I.
	11	40	i. Tr. E.
26	3	13	ii. Oc. D.
	6	43	i. Oc. D.
	8	7	ii. Ec. R.
	10	5	i. Ec. R.
27	3	55	i. Tr. I.
	6	8	i. Tr. E.
	15	59	iii. Tr. I.
	18	0	iii. Tr. E.
	21	58	ii. Tr. I.
28	0	29	ii. Tr. E.
	1	11	i. Oc. D.
	4	33	i. Ec. R.
	22	23	i. Tr. I.
29	0	36	i. Tr. E.
	16	28	ii. Oc. D.
	19	0	ii. Oc. R.
	19	1	ii. Ec. D.
	19	38	i. Oc. D.
	21	27	ii. Ec. R.
	23	2	i. Ec. R.
30	16	51	i. Tr. I.
	19	4	i. Tr. E.
31	6	3	iii. Oc. D.
	8	5	iii. Oc. R.
	11	7	iii. Ec. D.
	11	12	ii. Tr. I.
	12	57	iii. Ec. R.
	13	44	ii. Tr. E.
	14	6	i. Oc. D.
	17	31	i. Ec. R.

August.

d	h	m	
1	11	19	i. Tr. I.
	13	32	i. Tr. E.
2	5	44	ii. Oc. D.
	8	16	ii. Oc. R.
	8	19	ii. Ec. D.

August (con.).

d	h	m	
2	8	35	i. Oc. D.
	10	45	ii. Ec. R.
	11	59	i. Ec. R.
3	5	47	i. Tr. I.
	8	1	i. Tr. E.
	19	47	iii. Tr. I.
	21	50	iii. Tr. E.
4	0	27	ii. Tr. I.
	2	59	ii. Tr. E.
	3	3	i. Oc. D.
	6	28	i. Ec. R.
5	0	16	i. Tr. I.
	2	29	i. Tr. E.
	19	1	ii. Oc. D.
	21	31	i. Oc. D.
	21	33	ii. Oc. R.
	21	39	ii. Ec. D.
6	0	5	ii. Ec. R.
	0	57	i. Ec. R.
	18	44	i. Tr. I.
	20	57	i. Tr. E.
7	9	52	iii. Oc. D.
	11	57	iii. Oc. R.
	13	42	ii. Tr. I.
	15	6	iii. Ec. D.
	15	59	i. Oc. D.
	16	14	ii. Tr. E.
	16	57	iii. Ec. R.
	19	25	i. Ec. R.
8	13	13	i. Tr. I.
	15	26	i. Tr. E.
9	8	18	ii. Oc. D.
	10	27	i. Oc. D.
	10	50	ii. Oc. R.
	10	57	ii. Ec. D.
	13	23	ii. Ec. R.
	13	54	i. Ec. R.
10	7	41	i. Tr. I.
	9	54	i. Tr. E.
	23	39	iii. Tr. I.
11	1	45	iii. Tr. E.
	2	58	ii. Tr. I.
	4	56	i. Oc. D.
	5	30	ii. Tr. E.
	8	23	i. Ec. R.
12	2	10	i. Tr. I.
	4	23	i. Tr. E.
	21	35	ii. Oc. D.
	23	24	i. Oc. D.
13	0	8	ii. Oc. R.
	0	16	ii. Ec. D.
	2	42	ii. Ec. R.
	2	51	i. Ec. R.
	20	38	i. Tr. I.
	22	52	i. Tr. E.
14	13	45	iii. Oc. D.
	15	52	iii. Oc. R.
	16	14	ii. Tr. I.

August (con.).

d	h	m	
14	17	53	i. Oc. D.
	18	47	ii. Tr. E.
	19	5	iii. Ec. D.
	20	57	iii. Ec. R.
	21	20	i. Ec. R.
15	15	7	i. Tr. I.
	17	20	i. Tr. E.
16	10	54	ii. Oc. D.
	12	21	i. Oc. D.
	13	27	ii. Oc. R.
	13	35	ii. Ec. D.
	15	49	i. Ec. R.
	16	1	ii. Ec. R.
17	9	36	i. Tr. I.
	11	49	i. Tr. E.
18	3	35	iii. Tr. I.
	5	31	ii. Tr. I.
	5	44	iii. Tr. E.
	6	50	i. Oc. D.
	8	4	ii. Tr. E.
	10	18	i. Ec. R.
19	4	5	i. Tr. I.
	6	18	i. Tr. E.
20	0	12	ii. Oc. D.
	1	19	i. Oc. D.
	2	45	ii. Oc. R.
	2	54	ii. Ec. D.
	4	46	i. Ec. R.
	5	20	ii. Ec. R.
	22	34	i. Tr. I.
21	0	47	i. Tr. E.
	17	43	iii. Oc. D.
	18	48	ii. Tr. I.
	19	47	i. Oc. D.
	19	52	iii. Oc. R.
	21	21	ii. Tr. E.
	23	3	iii. Ec. D.
	23	15	i. Ec. R.
22	0	56	iii. Ec. R.
	17	2	i. Tr. I.
	19	16	i. Tr. E.
23	13	31	ii. Oc. D.
	14	16	i. Oc. D.
	16	5	ii. Oc. R.
	16	12	ii. Ec. D.
	17	44	i. Ec. R.
	18	38	ii. Ec. R.
24	11	31	i. Tr. I.
	13	45	i. Tr. E.
25	7	36	iii. Tr. I.
	8	6	ii. Tr. I.
	8	45	i. Oc. D.
	9	46	iii. Tr. E.
	10	39	ii. Tr. E.
	12	12	i. Ec. R.
26	6	0	i. Tr. I.
	8	14	i. Tr. E.
27	2	51	ii. Oc. D.

August (con.).

d	h	m	
27	3	14	i. Oc. D.
	5	24	ii. Oc. R.
	5	32	ii. Ec. D.
	6	41	i. Ec. R.
	7	58	ii. Ec. R.
28	0	29	i. Tr. I.
	2	43	i. Tr. E.
	21	25	ii. Tr. I.
	21	43	i. Oc. D.
	21	45	iii. Oc. D.
	23	56	iii. Oc. R.
	23	58	ii. Tr. E.
29	1	10	i. Ec. R.
	3	2	iii. Ec. D.
	4	57	iii. Ec. R.
	18	59	i. Tr. I.
	21	12	i. Tr. E.
30	16	11	ii. Oc. D.
	16	12	i. Oc. D.
	18	45	ii. Oc. R.
	18	50	ii. Ec. D.
	19	38	i. Ec. R.
	21	16	ii. Ec. R.
31	13	28	i. Tr. I.
	15	41	i. Tr. E.

September.

d	h	m	
1	10	41	i. Oc. D.
	10	44	ii. Tr. I.
	11	40	iii. Tr. I.
	13	17	ii. Tr. E.
	13	52	iii. Tr. E.
	14	7	i. Ec. R.
2	7	57	i. Tr. I.
	10	11	i. Tr. E.
3	5	10	i. Oc. D.
	5	32	ii. Oc. D.
	8	5	ii. Oc. R.
	8	9	ii. Ec. D.
	8	36	i. Ec. R.
	10	36	ii. Ec. R.
4	2	26	i. Tr. I.
	4	40	i. Tr. E.
	23	39	i. Oc. D.
5	0	3	ii. Tr. I.
	1	50	iii. Oc. D.
	2	36	ii. Tr. E.
	3	4	i. Ec. R.
	4	3	iii. Oc. R.
	7	1	iii. Ec. D.
	8	57	iii. Ec. R.
	20	56	i. Tr. I.
	23	9	i. Tr. E.
6	18	8	i. Oc. D.
	18	53	ii. Oc. D.
	21	27	ii. Oc. R.
	21	28	ii. Ec. D.

September (con.).

d	h	m	
6	21	33	i. Ec. R.
	23	54	ii. Ec. R.
7	15	25	i. Tr. I.
	17	39	i. Tr. E.
8	12	38	i. Oc. D.
	13	23	ii. Tr. I.
	15	47	iii. Tr. I.
	15	56	ii. Tr. E.
	16	2	i. Ec. R.
	18	1	iii. Tr. E.
9	9	55	i. Tr. I.
	12	8	i. Tr. E.
10	7	7	i. Oc. D.
	8	15	ii. Oc. D.
	10	31	i. Ec. R.
	13	13	ii. Ec. R.
11	4	24	i. Tr. I.
	6	38	i. Tr. E.
12	1	36	i. Oc. D.
	2	43	ii. Tr. I.
	4	59	i. Ec. R.
	5	16	ii. Tr. E.
	6	0	iii. Oc. D.
	8	14	iii. Oc. R.
	11	1	iii. Ec. D.
	12	57	iii. Ec. R.
	22	54	i. Tr. I.
13	1	7	i. Tr. E.
	20	6	i. Oc. D.
	21	36	ii. Oc. D.
	23	28	i. Ec. R.
14	2	32	ii. Ec. R.
	17	23	i. Tr. I.
	19	37	i. Tr. E.
15	14	35	i. Oc. D.
	16	3	ii. Tr. I.
	17	57	i. Ec. R.
	18	37	ii. Tr. E.
	19	58	iii. Tr. I.
	22	13	iii. Tr. E.
16	11	53	i. Tr. I.
	14	7	i. Tr. E.
17	9	5	i. Oc. D.
	10	59	ii. Oc. D.
	12	25	i. Ec. R.
	15	51	ii. Ec. R.
18	6	23	i. Tr. I.
	8	36	i. Tr. E.
19	3	34	i. Oc. D.
	5	24	ii. Tr. I.
	6	54	i. Ec. R.
	7	58	ii. Tr. E.
	10	12	iii. Oc. D.
	12	27	iii. Oc. R.
	15	0	iii. Ec. D.
	16	57	iii. Ec. R.
20	0	52	i. Tr. I.
	3	6	i. Tr. E.

September (con.).

d	h	m	
20	22	4	i. Oc. D.
21	0	21	ii. Oc. D.
	1	23	i. Ec. R.
	5	9	ii. Ec. R.
	19	22	i. Tr. I.
	21	36	i. Tr. E.
22	16	34	i. Oc. D.
	18	46	ii. Tr. I.
	19	51	i. Ec. R.
	21	19	ii. Tr. E.
23	0	12	iii. Tr. I.
	2	28	iii. Tr. E.
	13	52	i. Tr. I.
	16	6	i. Tr. E.
24	11	3	i. Oc. D.
	13	44	ii. Oc. D.
	14	20	i. Ec. R.
	18	29	ii. Ec. R.
25	8	22	i. Tr. I.
	10	36	i. Tr. E.
26	5	33	i. Oc. D.
	8	7	ii. Tr. I.
	8	49	i. Ec. R.
	10	41	ii. Tr. E.
	14	27	iii. Oc. D.
	16	43	iii. Oc. R.
	18	58	iii. Ec. D.
	20	57	iii. Ec. R.
27	2	52	i. Tr. I.
	5	5	i. Tr. E.
28	0	3	i. Oc. D.
	3	7	ii. Oc. D.
	3	17	i. Ec. R.
	7	47	ii. Ec. R.
	21	21	i. Tr. I.
	23	35	i. Tr. E.
29	18	32	i. Oc. D.
	21	29	ii. Tr. I.
	21	46	i. Ec. R.
30	0	3	ii. Tr. E.
	4	29	iii. Tr. I.
	6	46	iii. Tr. E.
	15	51	i. Tr. I.
	18	5	i. Tr. E.

October.

d	h	m	
1	13	2	i. Oc. D.
	16	15	i. Ec. R.
	16	30	ii. Oc. D.
	21	6	ii. Ec. R.
2	10	21	i. Tr. i.
	12	35	i. Tr. E.
3	7	32	i. Oc. D.
	10	44	i. Ec. R.
	10	52	ii. Tr. I.
	13	26	ii. Tr. E.
	18	44	iii. Oc. D.

October (con.).

d	h	m	
3	21	2	iii. Oc. R.
	22	57	iii. Ec. D.
4	0	57	iii. Ec. R.
	4	51	i. Tr. I.
	7	5	i. Tr. E.
5	2	2	i. Oc. D.
	5	12	i. Ec. R.
	5	54	ii. Oc. D.
	10	24	ii. Ec. R.
	23	21	i. Tr. I.
6	1	35	i. Tr. E.
	20	32	i. Oc. D.
	23	41	i. Ec. R.
7	0	14	ii. Tr. I.
	2	48	ii. Tr. E.
	8	49	iii. Tr. I.
	11	7	iii. Tr. E.
	17	51	i. Tr. I.
	20	5	i. Tr. E.
8	15	2	i. Oc. D.
	18	10	i. Ec. R.
	19	18	ii. Oc. D.
	23	43	ii. Ec. R.
9	12	21	i. Tr. I.
	14	35	i. Tr. E.
10	9	32	i. Oc. D.
	12	38	i. Ec. R.
	13	37	ii. Tr. I.
	16	11	ii. Tr. E.
	23	4	iii. Oc. D.

October (con.).

d	h	m	
11	1	22	iii. Oc. R.
	2	55	iii. Ec. D.
	4	56	iii. Ec. R.
	6	51	i. Tr. I.
	9	5	i. Tr. E.
12	4	2	i. Oc. D.
	7	7	i. Ec. R.
	8	42	ii. Oc. D.
	13	2	ii. Ec. R.
13	1	21	i. Tr. I.
	3	36	i. Tr. E.
	22	32	i. Oc. D.
14	1	36	i. Ec. R.
	3	1	ii. Tr. I.
	5	35	ii. Tr. E.
	13	10	iii. Tr. I.
	15	29	iii. Tr. E.
	19	51	i. Tr. I.
	22	6	i. Tr. E.
15	17	2	i. Oc. D.
	20	4	i. Ec. R.
	22	6	ii. Oc. D.
16	2	21	ii. Ec. R.
	14	22	i. Tr. I.
	16	36	i. Tr. E.
17	11	32	i. Oc. D.
	14	33	i. Ec. R.
	16	24	ii. Tr. I.
	18	58	ii. Tr. E.
18	3	25	iii. Oc. D.

October (con.).

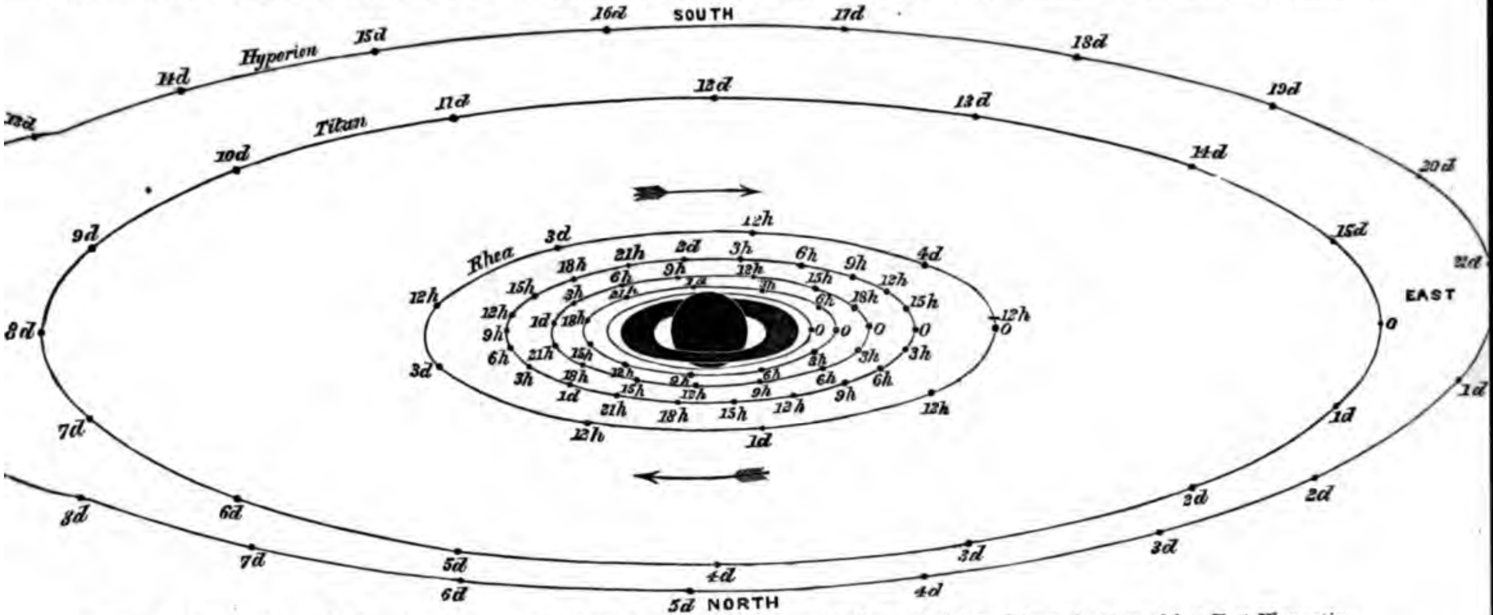
d	h	m	
18	5	45	iii. Oc. R.
	6	54	iii. Ec. D.
	8	52	i. Tr. I.
	8	56	iii. Ec. R.
	11	6	i. Tr. E.
19	6	2	i. Oc. D.
	9	2	i. Ec. R.
	11	31	ii. Oc. D.
	15	39	ii. Ec. R.
20	3	22	i. Tr. I.
	5	36	i. Tr. E.
21	0	32	i. Oc. D.
	3	30	i. Ec. R.
	5	48	ii. Tr. I.
	8	22	ii. Tr. E.
	17	33	iii. Tr. I.
	19	53	iii. Tr. E.
	21	52	i. Tr. I.
22	0	6	i. Tr. E.
	19	2	i. Oc. D.
	21	59	i. Ec. R.
23	0	55	ii. Oc. D.
	4	58	ii. Ec. R.
	16	22	i. Tr. I.
	18	37	i. Tr. E.
24	13	32	i. Oc. D.
	16	27	i. Ec. R.
	19	12	ii. Tr. I.
	21	46	ii. Tr. E.
25	7	49	iii. Oc. D.

October (con.).

d	h	m	
25	10	10	iii. Oc. R.
	10	53	i. Tr. I.
	10	53	iii. Ec. D.
	12	56	iii. Ec. R.
	13	7	i. Tr. E.
26	8	2	i. Oc. D.
	10	56	i. Ec. R.
	14	20	ii. Oc. D.
	18	16	ii. Ec. R.
27	5	23	i. Tr. I.
	7	37	i. Tr. E.
28	2	33	i. Oc. D.
	5	25	i. Ec. R.
	8	35	ii. Tr. I.
	11	9	ii. Tr. E.
	21	56	iii. Tr. I.
	23	53	i. Tr. I.
29	0	18	iii. Tr. E.
	2	8	i. Tr. E.
	21	3	i. Oc. D.
	23	53	i. Ec. R.
30	3	45	ii. Oc. D.
	7	34	ii. Ec. R.
	18	24	i. Tr. I.
	20	38	i. Tr. E.
31	15	33	i. Oc. D.
	18	22	i. Ec. R.
	21	59	ii. Tr. I.

Satellites I. and II. *disappear* at eclipse on the *p* side of the planet from January 1 to May 20, the distance between each satellite and the planet at the time of the disappearance of the former being nearly the same till the middle of April, when it rapidly decreases. Both satellites *reappear* on the *f* side from May 20 to the end of October. Satellite III. both *disappears* and *reappears* *p* from January 1 to April 30. The *disappearance* only is seen *p* from May 1 to May 20, and the *reappearance* only *f* from May 28 to June 4; and from June 11 to the end of October the satellite both *disappears* and *reappears* on the following side of the planet. Satellite IV. does not suffer occultation or eclipse, nor does it transit across the disk of Jupiter at any time throughout the year.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

SATELLITES OF SATURN.

Greenwich Mean Times of East Elongations.

MIMAS.

January.	February.	March.	April.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h
1 5·2	1 7·4	1 12·6	1 15·0	15 15·8	10 2·6	6 11·8
2 3·8	2 6·1	2 11·2	2 13·6	16 14·4	11 1·2	7 10·4
3 2·5	3 4·7	3 9·8	3 12·3	17 13·0	11 23·8	8 9·0
4 1·1	4 3·3	4 8·4	4 10·9	18 11·6	12 22·4	9 7·6
4 23·8	5 1·9	5 7·0	5 9·5	19 10·2	13 21·0	10 6·3
5 22·4	6 0·5	6 5·6	6 8·1	20 8·8	14 19·6	11 4·9
6 21·0	6 23·1	7 4·2	7 6·7	21 7·4	15 18·2	12 3·5
7 19·6	7 21·8	8 2·9	8 5·3	22 6·0	16 16·8	13 2·1
8 18·2	8 20·5	9 1·5	9 3·9	23 4·6	17 15·4	14 0·8
9 16·8	9 19·1	10 0·1	10 2·5	24 3·3	18 14·0	14 23·4
10 15·4	10 17·7	10 22·8	11 1·1	25 1·9	19 12·7	15 22·0
11 14·0	11 16·3	11 21·4	11 23·8	26 0·5	20 11·3	16 20·6
12 12·6	12 14·9	12 20·0	12 22·4	26 23·2	21 9·9	17 19·2
13 11·2	13 13·5	13 18·6	13 21·0	27 21·8	22 8·6	18 17·8
14 9·8	14 12·1	14 17·3	14 19·7	28 20·4	23 7·2	19 16·4
15 8·4	15 10·7	15 15·9	15 18·3	29 19·1	24 5·8	20 15·0
16 7·0	16 9·3	16 14·5	16 16·9	30 17·7	25 4·4	21 13·6
17 5·6	17 7·9	17 13·1	17 15·5	31 16·3	26 3·0	22 12·3
18 4·2	18 6·5	18 11·7	18 14·1		27 1·6	23 10·9
19 2·9	19 5·1	19 10·3	19 12·7		28 0·2	24 9·5
20 1·5	20 3·7	20 9·0	20 11·4		28 22·8	25 8·1
21 0·2	21 2·4	21 7·6	21 10·0	November.	29 21·4	26 6·7
21 22·8	22 1·0	22 6·2	22 8·6		30 20·1	27 5·3
22 21·4	22 23·6	23 4·8	23 7·2	1 15·0		28 4·0
23 20·0	23 22·2	24 3·4	24 5·9	2 13·6		29 2·6
24 18·6	24 20·8	25 2·0	25 4·5	3 12·2	December.	30 1·2
25 17·2	25 19·5	26 0·6	26 3·1	4 10·8		30 23·8
26 15·8	26 18·1	26 23·2	27 1·7	5 9·5	1 18·7	31 22·4
27 14·4	27 16·8	27 21·9	28 0·3	6 8·1	2 17·3	
28 13·0	28 15·4	28 20·5	28 23·0	7 6·7	3 16·0	
29 11·6	29 14·0	29 19·2	29 21·6	8 5·3	4 14·6	
30 10·2		30 17·8	30 20·2	9 3·9	5 13·2	
31 8·8		31 16·4				

ENCELADUS.

January.	February.	March.	April.	May.	November.	December.
d h	d h	d h	d h	d h	d h	d h
1 5·0	1 17·1	1 11·6	1 23·8	2 3·5	1 20·0	1 23·6
2 13·9	3 2·0	2 20·3	3 8·7	3 12·4	3 4·9	3 8·4
3 22·8	4 10·9	4 5·3	4 17·6	4 21·3	4 13·8	4 17·3
5 7·7	5 19·7	5 14·1	6 2·5	6 6·2	5 22·7	6 2·2
6 16·6	7 4·6	6 23·1	7 11·4	7 15·1	7 7·6	7 11·1
8 1·5	8 13·5	8 8·1	8 20·3	9 0·0	8 16·5	8 20·0
9 10·3	9 22·4	9 16·9	10 5·2	10 8·9	10 1·4	10 4·9
10 19·2	11 7·3	11 1·7	11 14·1		11 10·3	11 13·8
12 4·1	12 16·1	12 10·6	12 23·0		12 19·2	12 22·6
13 13·0	14 1·0	13 19·5	14 7·9		14 4·1	14 7·5
14 21·9	15 9·8	15 4·4	15 16·8	October.	15 13·0	15 16·4
16 6·7	16 18·7	16 13·3	17 1·7		16 21·9	17 1·2
17 15·6	18 3·6	17 22·1	18 10·6	19 3·2	18 6·8	18 10·1
19 0·5	19 12·4	19 7·0	19 19·5	20 12·1	19 15·6	19 19·0
20 9·3	20 21·3	20 15·9	21 4·4	21 21·0	21 0·5	21 3·9
21 18·2	22 6·2	22 0·9	22 13·3	23 5·8	22 9·4	22 12·8
23 3·0	23 15·1	23 9·7	23 22·2	24 14·7	23 18·3	23 21·7
24 11·9	25 0·0	24 18·6	25 7·1	25 23·6	25 3·2	25 6·6
25 20·8	26 8·9	26 3·5	26 16·0	27 8·5	26 12·0	26 15·4
27 5·7	27 17·8	27 12·4	28 0·8	28 17·4	27 20·9	28 0·3
28 14·6	29 2·6	28 21·2	29 9·7	30 2·3	29 5·8	29 9·2
29 23·4		30 6·1	30 18·6	31 11·2	30 14·7	30 18·0
		31 [illegible]				

Greenwich Mean Times of East Elongations.

TETHYS.

January.	February.	March.	April.	May.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h	d h
2 15·0	1 19·4	1 2·8	2 4·9	2 10·1	2 10·3	1 15·5	1 20·4
4 12·3	3 16·7	3 0·1	4 2·2	4 7·4	4 7·6	3 12·9	3 17·7
6 9·6	5 14·0	4 21·4	5 23·5	6 4·7	6 4·9	5 10·2	5 15·0
8 6·9	7 11·3	6 18·8	7 20·8	8 2·0	8 2·3	7 7·6	7 12·4
10 4·1	9 8·6	8 16·1	9 18·2	9 23·3	9 23·6	9 4·9	9 9·7
12 1·4	11 5·9	10 13·3	11 15·5	11 20 6	11 21·0	11 2·2	11 7·0
13 22·7	13 3·2	12 10·6	13 12·8		13 18·3	12 23·5	13 4·3
15 20·0	15 0·5	14 7·9	15 10·1		15 15·7	14 20·8	15 1·6
17 17·2	16 21·8	16 5·2	17 7·5		17 13·0	16 18·1	16 22·9
19 14·5	18 19·1	18 2·5	19 4·8		19 10·3	18 15·4	18 20·2
21 11·8	20 16·4	19 23·8	21 2·1	September.	21 7·6	20 12·7	20 17·5
23 9·1	22 13·7	21 21·1	22 23·4	30 13·0	23 4·9	22 10·1	22 14·7
25 6·4	24 10·9	23 18·4	24 20·8		25 2·2	24 7·4	24 12·0
27 3·6	26 8·2	25 15·7	26 18·1		26 23·5	26 4·6	26 9·3
29 0·9	28 5·5	27 13·0	28 15·4		28 20·8	28 1·9	28 6·6
30 22·1		29 10·3	30 12·7		30 18·2	29 23·1	30 3·9
		31 7·6					

DIONE.

January.	February.	March.	April.	May.	November.	December.
d h	d h	d h	d h	d h	d h	d h
1 10·9	3 6·6	1 15·1	3 11·1	3 13·9	3 2·9	3 5·5
4 4·5	6 0·3	4 8·8	6 4·8	6 7·6	5 20·6	5 23·1
6 22·2	8 17·9	7 2·5	8 22·6	9 1·3	8 14·3	8 16·8
9 15·9	11 11·5	9 20·2	11 16·3		11 8·0	11 10·4
12 9·5	14 5·1	12 13·8	14 10·0		14 1·7	14 4·1
15 3·2	16 22·8	15 7·5	17 3·7		16 19·3	16 21·7
17 20·8	19 16·4	18 1·1	19 21·4	October.	19 12·9	19 15·4
20 14·5	22 10·1	20 18·8	22 15·1	28 15·5	22 6·7	22 9·1
23 8·1	25 3·7	23 12·4	25 8·8	31 9·1	25 0·4	25 2·8
26 1·7	27 21·4	26 6·1	28 2·5		27 18·1	27 20·5
28 19·3		28 23·8	30 20·2		30 11·8	30 14·2
31 13·0		31 17·5				

RHEA.

January.	February.	March.	April.	May.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h	d h
2 12·9	3 3·1	1 5·1	1 19·9	3 11·2	8 20·1	4 23·2	2 1·7
7 1·2	7 15·4	5 17·5	6 8·3	7 23·7	13 8·6	9 11·6	6 14·1
11 13·6	12 3·8	10 5·8	10 20·8		17 21·2	14 0·1	11 2·4
16 1·9	16 16·1	14 18·2	15 9·2		22 9·7	18 12·5	15 14·8
20 14·2	21 4·4	19 6·6	19 21·7		26 22·2	23 0·9	20 3·1
25 2·5	25 16·8	23 19·0	24 10·2		31 10·7	27 13·3	24 15·4
29 14·8		28 7·4	28 22·7				29 3·8

TITAN.

January.	February.	March.	April.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h
11 15	12 10	15 4	16 2	25 10	10 9	12 6
27 12	28 7	31 2			26 8	28 4

Greenwich Mean Times of East Elongations.

HYPERION.

January.	February.	March.	April.	May.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h	d h
2 20	14 4	6 8	17 20	9 4	27 15	18 0	9 5
24 0		27 13					30 10

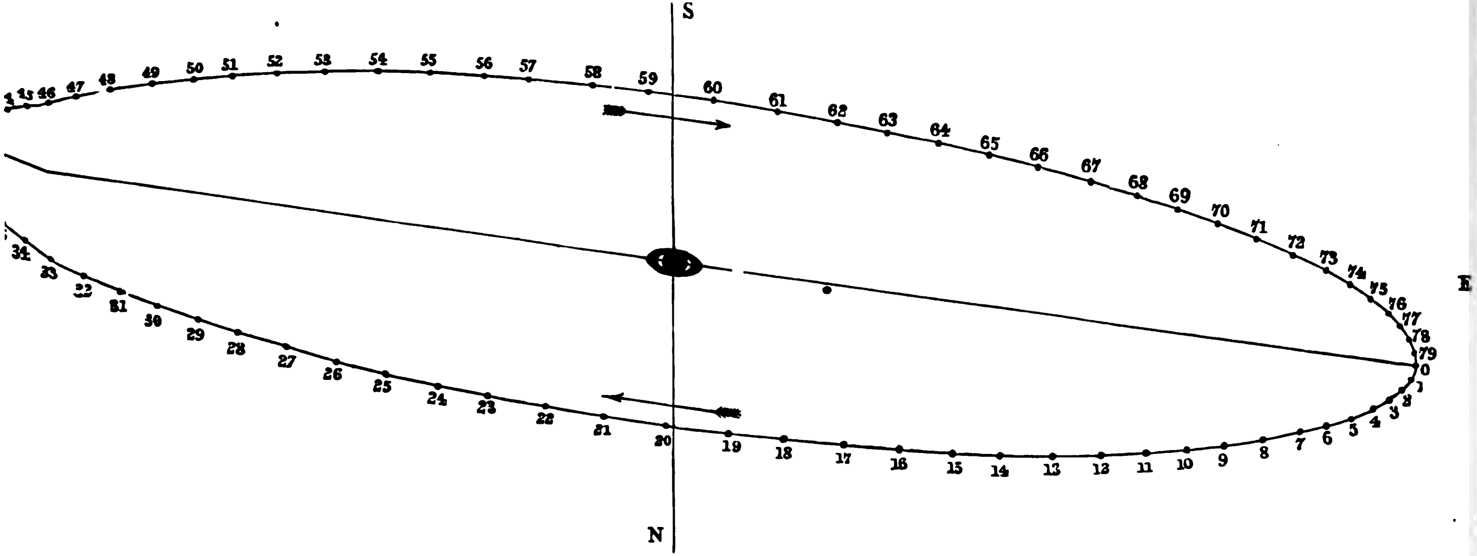
Apparent Elements of Saturn's Rings.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Latitude above Plane of Ring. Earth.	Latitude above Plane of Ring. Sun.
	° ′	″	″	° ′	° ′
Jan. 0	352 34·3	45·80	15·10	19 15·4 S.	20 9·9 S.
20	352 32·7	46·26	15·71	19 51·1 S.	19 56·1 S.
Feb. 9	352 31·3	46·01	16·08	20 27·2 S.	19 42·1 S.
29	352 30·5	45·11	16·12	20 56·0 S.	19 28·0 S.
Mar. 20	352 30·1	43·76	15·83	21 12·1 S.	19 13·7 S.
April 9	352 30·1	42·23	15·28	21 13·2 S.	18 59·2 S.
29	352 30·3	40·71	14·58	20 59·5 S.	18 44·6 S.
May 19	352 31·2	39·36	13·81	20 32·2 S.	18 29·8 S.
June 8	352 32·7	38·28	13·03	19 53·6 S.	18 14·8 S.
28	352 35·0	37·52	12·28	19 5·8 S.	17 59·8 S.
July 18	352 38·4	37·09	11·58	18 11·4 S.	17 44·5 S.
Aug. 7	352 42·7	37·01	10·96	17 13·3 S.	17 29·1 S.
27	352 47·7	37·27	10·43	16 14·6 S.	17 13·6 S.
Sept. 16	352 53·0	37·89	10·01	15 19·3 S.	16 57·9 S.
Oct. 6	352 58·0	28·84	9·74	14 31·4 S.	16 42·0 S.
26	353 2·1	40·08	9·64	13 55·5 S.	16 26·1 S.
Nov. 15	353 4·6	41·53	9·76	13 35·2 S.	16 10·1 S.
Dec. 5	353 5·1	43·06	10·10	13 33·4 S.	15 53·9 S.
25	353 3·5	44·45	10·64	13 50·9 S.	15 37·5 S.

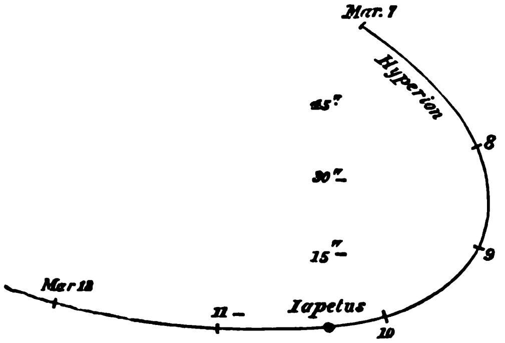
In the 'Monthly Notices,' p. 567, Mr. Marth says:—"Careful observations of the position-angles of the satellites at the times of their passing the direction of the minor axis of the ring afford the best means for determining the longitude of the satellites in their orbits, and are less liable to systematic errors, so that their evidence must be of very great weight in settling any questions concerning the orbital longitude."

He has also kindly furnished us with the diagrams on p. 34, representing the changes of position of Hyperion with reference to Titan and Iapetus, at its near approaches to these satellites. The motions of Titan and Iapetus being comparatively well known, observations of distance and position-angles between them and Hyperion at the times indicated are especially useful for correcting the tables of Hyperion.

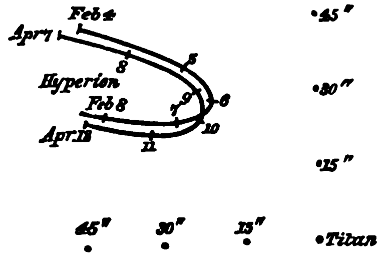
APPARENT ORBIT OF IAPETUS, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of motion. The figures indicate the number of days from the time of the last East Elongation.
Dates of East Elongations of Iapetus:—1887, December 5; 1888, February 21, May 10, July 31, October 20.

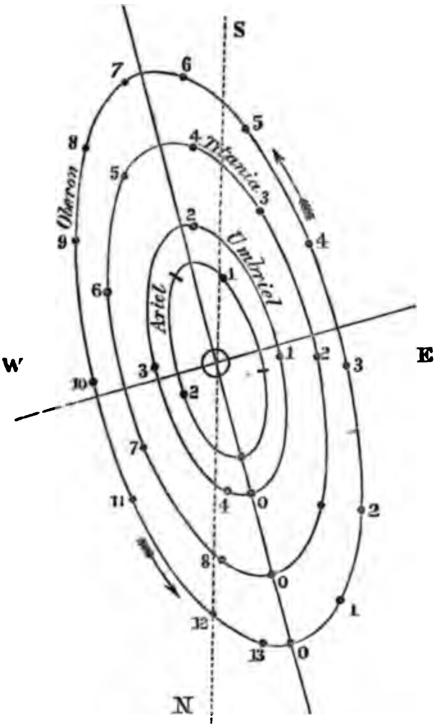


Conjunction of Hyperion and Iapetus, Mar. 10.



Nearest Approaches of Hyperion and Titan
Feb. 6 and Apr. 10

SATELLITES OF URANUS.



Apparent Orbits of the Satellites of Uranus, as seen in an inverting telescope.

The small circle in the above diagram represents the planet and is on the same scale as the orbits. The arrows show the direction of the motion of the Satellites, and the figures indicate the number of days from the time of the last North Elongation.

Greenwich Mean Times of North Elongations.

ARIEL.

January.		February.		March.		April.		May.		June.	
d	h	d	h	d	h	d	h	d	h	d	h
1	13·9	3	8·3	2	1·5	1	7·4	1	13·2	3	7·6
4	2·4	5	20·8	4	14·0	3	19·9	4	1·7	5	20·1
6	14·9	8	9·2	7	2·5	6	8·4	6	14·2	8	8·6
9	3·4	10	21·7	9	15·0	8	20·8	9	2·7	10	21·1
11	15·9	13	10·1	12	3·5	11	9·3	11	15·2	13	9·6
14	4·4	15	22·6	14	16·0	13	21·8	14	3·7	15	22·1
16	16·8	18	11·1	17	4·5	16	10·3	16	16·2	18	10·6
19	5·3	20	23·6	19	17·0	18	22·8	19	4·7	20	23·1
21	17·8	23	12·1	22	5·5	21	11·3	21	17·2	23	11·6
24	6·3	26	0·6	24	17·9	23	23·8	24	5·6	26	0·1
26	18·8	28	13·1	27	6·4	26	12·3	26	18·1	28	12·5
29	7·3			29	18·9	29	0·8	29	6·6	31	1·0
31	19·8							31	19·1		

UMBRIEL.

January.	February.	March.	April.	May.	June.
d h	d h	d h	d h	d h	d h
3 6·0	1 6·2	1 6·3	3 9·9	2 10·2	4 13·9
7 9·5	5 9·6	5 9·8	7 13·4	6 13·6	8 17·3
11 12·9	9 13·1	9 13·2	11 16·8	10 17·1	12 20·8
15 16·4	13 16·5	13 16·7	15 20·3	14 20·5	17 0·2
19 19·8	17 20·0	17 20·1	19 23·7	19 0·0	21 3·7
23 23·3	21 23·4	21 23·6	24 3·2	23 3·4	25 7·2
28 2·7	26 2·9	26 3·0	28 6·7	27 6·9	29 10·6
		30 6·4		31 10·4	

TITANIA.

January.	February.	March.	April.	May.	June.
3 8·5	7 4·2	4 6·9	8 2·6	4 5·4	8 1·2
12 1·5	15 21·1	12 23·8	16 19·5	12 22·4	16 18·1
20 18·4	24 14·0	21 16·8	25 12·5	21 15·3	25 11·1
29 11·3		30 9·7		30 8·2	34 4·0

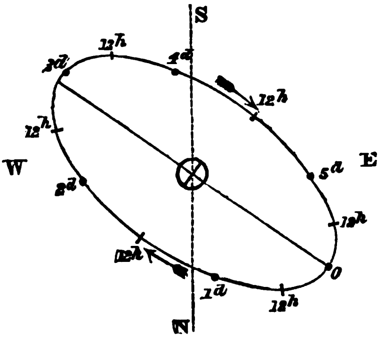
OBERON.

January.	February.	March.	April.	May.	June.
10 12·1	6 10·2	4 8·3	13 17·6	10 15·8	6 14·0
23 23·1	19 21·2	17 19·4	27 4·7	24 2·9	
		31 6·5			

The position-angle and distance of Titania and Oberon are:—

	TITANIA.		OBERON.	
	Angle.	Dist.	Angle.	Dist.
February 1	14°·3	33″·6	14°·3	45″·0
April 1	14°·2	34″·8	14°·2	46″·5
June 1	14°·1	33″·9	14°·1	45″·3

SATELLITE OF NEPTUNE.



Apparent Orbit of the Satellite of Neptune, as seen in an inverting telescope.

The small circle in the above diagram represents the planet, the arrows show the direction of motion, and the figures indicate the interval from the time of last North-east Elongation.

Greenwich Mean Times of North-East Elongations.

January.	February.	March.	September.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h
5 4·7	3 14·1	3 23·3	2 3·2	1 12·3	5 18·4	5 3·6
11 1·8	9 11·2	9 20·4	8 0·2	7 9·3	11 15·4	11 0·6
16 22·9	15 8·3	15 17·4	13 21·2	13 6·3	17 12·5	16 21·6
22 20·0	21 5·3	21 14·4	19 18·2	19 3·3	23 9·5	22 18·6
28 17·1	27 2·3		25 15·3	25 0·4	29 6·5	28 15·6
				30 21·4		

The position-angle and distance of the Satellite are as follows:—January 14, *°·7, 16″·7; September* 14, 237°·1, 16″·6; November 17, 236°·8, 17″·0.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon. 1888.	P.	D.	L.	Greenwich Mean Noon. 1888.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	1 56	+3 14	284 32	July 4	0 50	+3 30	2 42
6	0 30	3 48	218 42	9	1 26	4 0	296 32
11	2 56	4 20	152 52	14	3 41	4 30	230 22
16	5 18	4 50	87 1	19	5 54	4 57	164 13
21	7 36	5 17	21 11	24	8 4	5 23	98 4
26	9 51	5 43	315 21	29	10 8	5 46	31 56
31	11 58	6 5	249 31	Aug. 3	12 8	6 7	325 49
Feb. 5	13 59	6 24	183 41	8	14 2	6 25	259 42
10	15 53	6 41	117 51	13	15 50	6 40	193 36
15	17 38	6 54	52 1	18	17 30	6 53	127 31
20	19 15	7 4	346 10	23	19 4	7 3	61 27
25	20 43	7 11	280 19	28	20 29	7 10	355 23
Mar. 1	22 1	7 15	214 27	Sept. 2	21 47	7 14	289 21
6	23 10	7 15	148 34	7	22 55	7 15	223 19
11	24 9	7 11	82 41	12	23 55	7 13	157 18
16	24 59	7 5	16 47	17	24 46	7 7	91 17
21	25 37	6 56	310 51	22	25 27	6 58	25 18
26	26 6	6 42	244 55	27	25 58	6 47	319 19
31	26 23	6 27	178 58	Oct. 2	26 19	6 32	253 20
Apr. 5	26 30	6 8	112 59	7	26 29	6 14	187 22
10	26 26	5 47	46 59	12	26 28	5 53	121 25
15	26 11	5 22	340 58	17	26 16	5 30	55 28
20	25 44	4 57	274 55	22	25 52	5 4	349 31
25	25 7	4 29	208 51	27	25 17	4 36	283 35
30	24 19	3 59	142 47	Nov. 1	24 30	4 6	217 39
May 5	23 19	3 28	76 41	6	23 30	3 34	151 44
10	22 10	2 55	10 34	11	22 20	3 0	85 49
15	20 50	2 21	304 26	16	20 57	2 24	19 54
20	19 20	1 46	238 18	21	19 24	1 47	314 0
25	17 41	1 10	172 9	26	17 39	1 10	248 6
30	15 53	0 34	105 59	Dec. 1	15 46	+0 32	182 12
June 4	13 59	+0 2	39 48	6	13 43	−0 7	116 19
9	11 57	+0 38	333 37	11	11 33	0 45	50 26
14	9 50	1 14	267 26	16	9 16	1 23	344 34
19	7 39	1 49	201 15	21	6 54	2 1	278 42
24	5 24	2 24	135 4	26	4 30	2 37	212 50
29	3 8	+2 57	68 53	31	2 4	−3 12	146 59

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74°. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

WITHDRAWN

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF MARS, 1888.

Greenwich Noon.	Angle of position of Mars' axis.	Areogr. Lat. of centre of disk.	Diam.	Greatest defect of illumination.	Angle of position of greatest defect.
	°	°	"	"	°
Jan. 31	35·08	+20·24	8·63	0·77	290·68
Feb. 20	36·04	+18·89	10·43	0·72	288·94
Mar. 11	36·15	+18·64	12·66	0·47	287·06
31	35·32	+19·92	14·74	0·09	281·60
Apr. 20	33·42	+22·13	15·43	0·07	120·71
May 10	31·65	+23·93	14·31	0·56	114·21
30	31·55	+24·79	12·44	1·01	113·09
June 19	33·12	+24·69	10·70	1·20	112·47
July 9	35·43	+23·55	9·33	1·21	111·57
29	37·43	+21·21	8·29	1·13	109·93

Approximate Greenwich Mean Times at which the Zero-Meridian passes the centre of the disk.

	d	h		d	h		d	h		d	h		d	h
Jan.	31	6·7	Mar.	8	5·5	Apr.	14	3·2	May	21	1·1	June	27	0·0
Feb.	2	8·0		10	6·7		16	4·4		23	2·3		29	1·3
	4	9·3		12	7·9		18	5·6		25	3·6	July	1	2·7
	6	10·5		14	9·1		20	6·8		27	4·8		3	4·0
	8	11·8		16	10·4		22	8·0		29	6·1		5	5·3
	10	13·1		18	11·6		24	9·2		31	7·3		7	6·6
	12	14·4		20	12·8		26	10·4	June	2	8·6		9	7·9
	14	15·6		22	14·0		28	11·6		4	9·9		11	9·2
	16	16·9		24	15·2		30	12·8		6	11·1		13	10·5
	18	18·2		26	16·4	May	2	14·0		8	12·4		15	11·8
	20	19·5		28	17·6		4	15·2		10	13·7		17	13·2
	22	20·7		30	18·8		6	16·4		12	15·0		19	14·5
	24	22·0	Apr.	1	20·0		8	17·6		14	16·3		21	15·8
	26	23·2		3	21·2		10	18.8		16	17·5		23	17·1
	29	0·5		5	22·4		12	20·1		18	18·8		25	18·4
Mar.	2	1·7		7	23·6		14	21·3		20	20·1		27	19·8
	4	3·0		10	0·8		16	22·6		22	21 4		29	21·1
	6	4·2		12	2·0		18	23·8		24	22·7		31	22·5

The above table is given for every alternate passage.

Intervals in Time by which certain Points on the Surface of Mars pass the Centre of the Disk after the Zero-Meridian.

No.	Mädler's letter.	Green's Name.		Schiaparelli's Name.	Areogr. Long.	Interval in time.
					°	h
1.	*a*	Dawes Forked Bay.		Vertice d'Aryn 1.	0·92	0·1
2.	*d*	Terby Sea.		Lacus Solis 14.	90·87	6·2
3.	*p*	Pratt Bay.	Maraldi Sea.	Mare Sirenum 19 *a*.	131·80	9·0
4.	*r*	Trouvelot Bay.		Gigantum 25.	170·17	11·6
5.	*n*	Huggins Bay.		Mare Cimmerium 41.	239·97	16·4
6.	*f*	Kaiser Sea.		Syrtis Major 51 *c*.	281·72	19·3
7.	*e*	Banks Cape.		Hammonis Cornu 57.	317·99	21·8

The Zero-Meridian adopted in this Ephemeris passes through the point of the tongue of the "Forked Bay" of Dawes. The assumed period of the sidereal *rotation of Mars is* $24^h\ 37^m\ 22^s{\cdot}63$.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF JUPITER, 1888.

Greenwich Noon.	Angle of position of Jupiter's axis.	Latitude of Earth above Jupiter's Equator.	Latitude of Sun above Jupiter's Equator.	Equatoreal Diameter.	Defect of illumination.
	°	°	°	"	"
Jan. 3 ……	13·189	−3·007	−3·056	32·32	0·13
23 ……	11·838	−3·038	−3·048	33·77	0·22
Feb. 12 ……	10·744	−3·070	−3·037	35·66	0·29
Mar. 3 ……	10·032	−3·101	−3·023	37·89	0·32
23 ……	9·801	−3·132	−3·008	40·31	0·27
Apr. 12 ……	10·089	−3·156	−2·990	42·60	0·17
May 2 ……	10·827	−3·160	−2·970	44·29	0·05
22 ……	11·817	−3·135	−2·948	44·97	0·00
June 11 ……	12·784	−3·078	−2·924	44·45	0·06
July 1 ……	13·481	−3·000	−2·896	42·90	0·18
21 ……	13·764	−2·917	−2·868	40·75	0·29
Aug. 10 ……	13·590	−2·840	−2·836	38·43	0·34
30 ……	12·975	−2·780	−2·803	36·26	0·32
Sept. 19 ……	11·959	−2·726	−2·767	34·40	0·25

Greenwich Mean Times at which the Zero Meridian for Observations of the Great Reddish Spot passes the middle of the Illuminated Disk.

January.			January.			February.			March.			April.			April.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
1	14	8·4	26	19	48·7	17	18	1·1	10	16	11·5	1	14	20·0	26	9	55·9
2	19	55·6	27	15	40·1	18	13	52·5	11	12	2·8	2	10	11·2	27	15	42·7
3	15	47·2	28	21	27·3	19	19	39·5	12	17	49·7	3	15	58·0	28	11	33·9
4	21	34·4	29	17	18·7	20	15	30·9	13	13	41·0	4	11	49·3	29	17	20·6
5	17	25·9	30	13	10·2	21	21	17·9	14	19	28·0	5	17	36·1	30	13	11·8
6	13	17·5	31	18	57·3	22	17	9·3	15	15	19·2	6	13	27·3			
7	19	4·7				23	13	0·6	16	21	6·2	7	19	14·1	May.		
8	14	56·2	February.			24	18	47·6	17	16	57·4	8	15	5·3			
9	20	43·5				25	14	39·0	18	12	48·7	9	10	56·6	1	9	3·0
10	16	35·0	1	14	48·8	26	20	26·0	19	18	35·6	10	16	43·4	2	14	49·7
11	22	22·2	2	20	35·9	27	16	17·4	20	14	26·9	11	12	34·6	3	10	40·9
12	18	13·7	3	16	27·3	28	12	8·7	21	20	13·8	12	18	21·4	4	16	27·7
13	14	5·2	4	12	18·7	29	17	55·7	22	16	5·0	13	14	12·6	5	12	18·8
14	19	52·4	5	18	5·9				23	11	56·3	14	10	3·8	6	8	10·0
15	15	43·9	6	13	57·3	March.			24	17	43·2	15	15	50·6	7	13	56·8
16	21	31·1	7	19	44·4				25	13	34·4	16	11	41·8	8	9	47·9
17	17	22·6	8	15	35·8	1	13	47·0	26	19	21·3	17	17	28·5	9	15	34·7
18	13	14·1	9	21	22·9	2	19	34·0	27	15	12·5	18	13	19·7	10	11	25·8
19	19	1·3	10	17	14·3	3	15	25·1	28	11	3·8	19	9	10·9	11	17	12·6
20	14	52·8	11	13	5·7	4	21	12·3	29	16	50·6	20	14	57·7	12	13	3·7
21	20	40·0	12	18	52·8	5	17	3·7	30	12	41·9	21	10	48·9	13	8	54·9
22	16	31·4	13	14	44·2	6	12	55·0	31	18	28·7	22	16	35·6	14	14	41·7
23	12	22·9	14	20	31·3	7	18	41·9				23	12	26·8	15	10	32·8
24	18	10·1	15	16	22·6	8	14	33·2				24	18	13·6	16	16	19·6
25	14	1·5	16	12	14·0	9	20	20·2				25	14	4·8	17	12	10·8

Ephemeris for Jupiter (*continued*).

Greenwich Mean Times at which the Zero Meridian for observations of the Great Reddish Spot passes the middle of the Illuminated Disk. (1888). (*Continued*).

May.			June.			July.			July.			August.			September.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
18	8	1·9	8	10	17·5	1	14	13·7	24	8	17·5	13	4	53·6	1	5	39·9
19	13	48·7	9	6	8·8	2	10	5·1	25	14	4·7	14	10	41·0	2	11	27·4
20	9	39·9	10	11	55·6	3	5	56·5	26	9	56 2	15	6	32·5	3	7	19·0
21	15	26·6	11	7	46·9	4	11	43·5	27	5	47·6	16	12	19·9	4	13	6·4
22	11	17·8	12	13	33·8	5	7	34·9	28	11	34·9	17	8	11·4	5	8	58·0
23	7	9·0	13	9	25·0	6	13	21·9	29	7	26·3	18	13	58·8	6	4	49·6
24	12	55·8	14	15	11·9	7	9	13·3	30	13	13·6	19	9	50·3	7	10	37·1
25	8	47·0	15	11	3·2	8	15	0·4	31	9	5·1	20	5	41·9	8	6	28·7
26	14	33·7	16	6	54·5	9	10	51·8				21	11	29·3	9	12	16·1
27	10	24·9	17	12	41·4	10	6	43·2	August.			22	7	20·8	10	8	7·7
28	16	11·7	18	8	32·7	11	12	30·3				23	13	8·2	11	3	59·4
29	12	2·9	19	14	19·6	12	8	21·7	1	4	56·6	24	8	59·8	12	9	46·8
30	7	54·1	20	10	10·9	13	14	8·8	2	10·	43·8	25	4	51·4	13	5	38·5
31	13	40·9	21	6	2·2	14	10	0·2	3	6	35·3	26	10	38·8	14	11	25·9
			22	11	49·1	15	5	51·6	4	12	22·6	27	6	30·4	15	7	17·6
June.			23	7	40·4	16	11	38·7	5	8	14·1	28	12	17·7	16	13	5·0
1	9	32·1	24	13	27·4	17	7	30·2	6	14	1·4	29	8	9·3	17	8	56·7
2	15	18·9	25	9	18·7	18	13	17·3	7	9	52·9	30	13	56·7	18	4	48·3
3	11	10·2	26	15	5·7	19	9	8·8	8	5	44·4	31	9	48·3	19	10	35·8
4	7	1·4	27	10	57·0	20	5	0·2	9	11	31·7				20	6	27·4
5	12	48·2	28	6	48·4	21	10	47·4	10	7	23·2				21	12	14·9
6	8	39·4	29	12	35·4	22	6	38·8	11	13	10·6						
7	14	26·3	30	8	26·7	23	12	26·0	12	9	2·1						

STARS WITH REMARKABLE SPECTRA.

The great majority of the stars visible to the naked eye have spectra which may be grouped under one of two types:—(1) those spectra which are crossed by four broad dark ill-defined lines corresponding to hydrogen, and with few or no traces of metallic lines; and (2) those in which, as in the solar spectrum, the hydrogen lines are much less conspicuous, and which show a great number of narrow lines due to the metals. Amongst the brightest in each class we find:—

Class I.—α, μ Andromedæ; ζ Aquarii; α, δ, ζ, η, θ, λ Aquilæ; β Arietis; β, η, θ Aurigæ; γ Boötis; α, β, γ, ε, η Canis Majoris; β Canis Minoris; β, δ, ε Cassiopeiæ; α Cephei; γ Ceti; α, β, γ Coronæ Borealis; γ, δ Corvi; δ Cygni; α, ε Delphini; α, ζ Draconis, α, γ, δ, θ, ξ Geminorum; γ, δ, ε, μ Herculis; α, β, δ, ζ, η Leonis; α, ε, ζ, η Leporis; α, β Libræ; α, γ Lyræ; α, λ, γ Ophiuchi; α, γ, ζ Pegasi; α, β, γ, δ Persei; α Piscis Australis; α Piscium; β Scorpii; ε, μ Serpentis; β, η, ξ Tauri; β Trianguli; β, γ, δ, ε, ζ, η Ursæ Majoris; α, γ, ζ, η Virginis.

Class II.—β, γ, δ, λ, ο Andromedæ; α, β Aquarii; β, γ, ε Aquilæ; α Arietis; α, δ, ε, ζ, ι Aurigæ; α, β, δ, ε, η, ρ Boötis; δ Canis Majoris; α^1, α^2 Capricorni; α, ζ, η Cassiopeiæ; β, γ, ζ, η Cephei; β, ζ, η, θ, ι Ceti; β Corvi; α, β, γ, ε, ζ, τ Cygni; β Delphini; β, γ, η, θ, ι, ξ, χ Draconis; β Geminorum; β, ζ, η, π Herculis; α, ε, ζ Hydræ; γ, ε, μ Leonis; β, γ Leporis; β, ε, κ Ophiuchi; ε, η, μ Pegasi; ε, ζ Persei; η Piscium; δ Scorpii; α Serpentis; α, γ, δ′, ε, ο Tauri; α Trianguli; α Ursæ Majoris; β Ursæ Minoris; β, ε Virginis.

Procyon and Polaris seem to offer spectra of a type intermediate between the first and second classes.

The principal stars of Orion, with the exception of α Orionis, form a subdivision of Class I.; the hydrogen lines being still the principal lines of the spectrum, but far fainter and narrower than is usual in stars of the class.

α Orionis appears to present a spectrum intermediate between the second and a third class; the third class being those that are crossed by dark shaded *bands which terminate* for the most part in a very dark sharp edge towards the

STARS WITH REMARKABLE SPECTRA (*continued*).

violet, and gradually shade away towards the red. The brightest stars of this class are as follows:—

CLASS III.—λ, φ, χ Aquarii; π, ρ Arietis; π, υ Aurigæ; υ Boötis; μ Cephei; α, ο, υ Ceti; ν^1, ν^2 Coronæ; ω^3, χ Cygni; λ Draconis; γ, π Eridani; η, μ Geminorum; α Herculis; π, ψ Leonis; δ^2 Lyræ; δ Ophiuchi; ο′ Orionis; β, φ Pegasi; ρ Persei; δ Piscium; δ Sagittæ; α Scorpii; κ, τ′ Serpentis; ρ, μ Ursæ Majoris; δ, ψ, σ Virginis; α Vulpeculæ.

The following stars are also fine examples of the type.

The places are given for 1890·0.

Star's Name.	R.A.	Dec.	Star's Name.	R.A.	Dec.
	h m s	° ′		h m s	° ′
R Andromedæ	0 18 13	+37 58·1	DM +47° No. 2291	15 59 21	+47 32·5
20 Leporis	5 6 14	−11 59·0	*g* Herculis	16 25 1	+42 7·4
119 Tauri	5 25 46	+18 30·7	407 Birmingham ...	17 14 14	+ 2 15·2
51 Geminorum	7 7 4	+16 20·8	R Lyræ	18 51 59	+43 47·9
R Leonis	9 41 39	+11 56·4	569 Birmingham ...	20 40 26	+17 41·4
56 Leonis	10 50 19	+ 6 46·4	3 Aquarii	20 41 56	− 5 25·8
40 Comæ	13 1 1	+23 12·5	LL 42153	21 31 51	+44 52·9
R Hydræ	13 23 42	−22 42·7	DM +14° No. 4730	21 59 44	+14 16·9
169 Schjellerup......	14 19 15	+26 12·3	615 Birmingham ...	22 34 18	+56 13·5
LL 26918	14 40 35	+15 35·7	71 Pegasi	23 27 58	+21 53·6
342 Birmingham ...	14 55 50	+66 22·2	30 Piscium	23 56 19	− 6 37·5

R Andromedæ presents some differences from ordinary third type spectra.

The fourth class, which embraces those spectra in which the dark bands have their darkest edge on the side nearer the red, and shade away towards the violet, contains very few stars visible to the naked eye. The following are the most remarkable examples:—

Star's Name.	R.A.	Dec.	Star's Name.	R.A.	Dec.
	h m s	° ′		h m s	° ′
DM +57° No. 702 .	3 2 57	+57 29·1	DM +68° No. 617 .	10 37 26	+67 59·3
51 Schjellerup......	4 59 43	+ 1 1·5	152 Schjellerup......	12 39 58	+46 2·5
72 Schjellerup......	6 4 4	+26 2·1	155 *b* Schjellerup ...	12 52 6	+66 35·4
78 Schjellerup......	6 28 59	+38 32·0	229 Schjellerup......	19 25 26	+76 20·6
115 Schjellerup......	8 49 11	+17 39·0	249 *a* Schjellerup ...	21 37 23	+35 0·5
132 Schjellerup......	10 32 6	−12 48·8	19 Piscium	23 40 46	+ 2 52·7

Besides the above-named four types there are a few stars which show the lines of hydrogen bright in their spectra. Only four bright stars belong to this class—γ Cassiopeiæ, β Lyræ, and γ and η Argûs. The following small stars also have bright lines in their spectra.

Star's Name.	R.A.	Dec.	Star's Name.	R.A.	Dec.
	h m s	° ′		h m s	° ′
LL 13412	6 49 33	−23 47·3	DM +35° No. 3952	20 1 43	+35 21·6
R Geminorum	7 0 44	+22 52·4	DM +35° No. 4001	20 6 4	+35 51·1
* Argûs	8 51 19	−47 10·0	DM +35° No. 4013	20 7 42	+35 51·1
* Argûs	10 37 15	−58 10·8	DM +37° No. 3821	20 8 6	+38 1·5
Gould 15305.........	11 5 25	−60 22·9	DM +36° No. 3956	20 10 25	+36 19·5
* Centaurus	13 11 3	−57 33·2	DM +36° No. 3987	20 12 55	+37 4·9
Stone 9168	16 46 36	−41 38·2	P Cygni	20 13 44	+37 41·4
O.A. (S.) 17681......	18 1 46	−21 16·2	DM +38° No. 4010	20 15 31	+38 23·0

MEAN PLACES OF VARIABLE STARS FOR 1888·0.

Star.	R.A.	Dec.	Star.	R.A.	Dec.
	h m s	° ′		h m s	° ′
T Cassiopeiæ ...	0 17 11	+55 10·3	T Libræ	15 4 21	−19 35·5
R Andromedæ .	0 18 7	+37 57·4	U Coronæ	15 13 38	+32 3·4
S Ceti	0 18 22	− 9 56·9	S Libræ	15 14 58	−19 59·0
U Cephei	0 52 23	+81 16·3	S Serpentis......	15 16 25	+14 43·0
S Cassiopeiæ ...	1 11 26	+72 1·3	S Coronæ	15 16 50	+31 46·2
S Piscium	1 11 43	+ 8 20·4	V Coronæ	15 45 31	+39 54·5
R Sculptoris ...	1 21 49	−33 7·4	R Serpentis......	15 45 32	+15 28·5
R Piscium	1 24 51	+ 2 18·2	R Libræ	15 47 16	−15 54·1
S Arietis.........	1 58 37	+11 59·3	R Herculis	16 1 11	+18 40·4
R Arietis.........	2 9 45	+24 32·1	W Scorpii	16 5 13	−19 50·7
o Ceti	2 13 41	− 3 29·2	R Scorpii	16 10 58	−22 40·0
S Persei	2 14 49	+58 4·4	S Scorpii.........	16 11 0	−22 37·0
R Ceti............	2 20 18	− 0 41·1	W Ophiuchi ...	16 15 23	− 7 25·9
T Arietis.........	2 42 5	+17 2·5	V Ophiuchi ...	16 20 30	−12 9·7
Algol	3 0 53	+40 31·4	U Herculis	16 20 51	+19 8·7
R Persei	3 22 55	+35 17·3	T Ophiuchi......	16 27 20	−15 53·5
λ Tauri	3 54 29	+12 10·4	S Ophiuchi......	16 27 49	−16 55·4
R Tauri	4 22 10	+ 9 54·7	W Herculis......	16 31 15	+37 34·1
V Tauri	4 45 33	+17 20·9	R Ursæ Minor .	16 31 29	+72 30·3
R Orionis	4 52 56	+ 7 57·6	R Draconis......	16 32 22	+66 59·5
R Leporis	4 54 31	−14 58·5	S Herculis	16 46 47	+15 7·9
R Aurigæ	5 8 15	+53 27·6	R Ophiuchi......	17 1 20	−15 56·6
S Aurigæ.........	5 19 43	+34 3·0	U Ophiuchi......	17 10 51	+ 1 20·1
T Orionis	5 49 10	+20 9·2	X (3) Sagittarii.	17 40 30	−27 47·2
η Geminorum...	6 8 7	+22 32·3	W (γ′) Sagittarii	17 57 52	−29 35·1
T Monocerotis...	6 19 10	+ 7 8·8	T Herculis	18 4 52	+31 0·1
ζ Geminorum...	6 57 28	+20 44·0	Z Sagittarii......	18 14 48	−18 54·5
R Geminorum..	7 0 37	+22 52·6	T Serpentis......	18 23 21	+ 6 13·5
R Canis Minor .	7 2 33	+10 12·0	U Sagittarii ...	18 25 18	−19 12·3
L² Puppis	7 10 7	−44 27·6	R Scuti	18 41 30	− 5 49·5
V Geminorum ..	7 16 53	+13 18·4	β Lyræ	18 45 57	+33 14·0
U Monocerotis...	7 25 27	− 9 32·6	R Lyræ	18 51 56	+43 47·9
S Canis Minor .	7 26 38	+ 8 33·4	R Aquilæ	19 0 59	+ 8 3·7
T Canis Minor .	7 27 46	+11 59·0	T Sagittarii ...	19 9 46	−17 10·0
U Canis Minor .	7 35 16	+ 8 38·5	R Sagittarii ...	19 10 8	−19 3·3
S Geminorum...	7 36 19	+23 42·8	S Sagittarii......	19 12 53	−19 13·6
T Geminorum...	7 42 35	+24 0·7	U Aquilæ	19 23 20	− 7 16·4
R Cancri.........	8 10 23	+12 4·2	R Cygni	19 33 49	+49 57·0
V Cancri.........	8 15 20	+17 38·4	S Vulpeculæ ...	19 43 48	+27 0·5
U Cancri.........	8 29 22	+19 16·9	χ Cygni	19 46 16	+32 37·9
S Cancri	8 37 32	+19 26·2	*η* Aquilæ.........	19 46 46	+ 0 43·1
S Hydræ.........	8 47 43	+ 3 29·5	S (10) Sagittæ .	19 50 55	+16 20·2
T Hydræ.........	8 50 13	− 8 42·9	S Cygni	20 3 9	+57 39·8
T Cancri.........	8 50 17	+20 16·7	R Capricorni ...	20 5 1	−14 36·0
R Carini	9 29 26	−62 17·6	S Aquilæ.........	20 6 28	+15 17·3
R Leo. Minor. .	9 38 52	+35 1·6	R Sagittæ	20 8 57	+16 23·2
R Leonis.........	9 41 32	+11 56·9	R Delphini......	20 9 31	+ 8 45·0
U Hydræ	10 32 1	−12 48·1	U Cygni	20 16 8	+47 32·5
R Ursæ Major .	10 36 43	+69 21·8	V Cygni	20 37 42	+47 44·5
R Crateris	10 55 3	−17 43·4	S Delphini	20 37 55	+16 41·2
S Leonis	11 5 3	+ 6 4·1	X Cygni	20 39 1	+35 11·0
R Comæ	11 58 31	+19 24·5	T Delphini	20 40 10	+15 59·5
T Virginis	12 8 52	− 5 24·8	U Capricorni ...	20 41 54	−15 11·7
R Corvi	12 13 50	−18 37·9	T Aquarii	20 44 2	− 5 33·7
T Ursæ Major .	12 31 17	+60 6·2	T Vulpeculæ ...	20 46 43	+27 49·6
R Virginis	12 32 49	+ 7 36·3	Y Cygni	20 47 36	+34 14·2
S Ursæ Major..	12 39 3	+61 42·4	R Vulpeculæ ...	20 59 25	+23 22·7
U Virginis	12 45 25	+ 6 9·8	T Cephei.........	21 8 4	+68 2·1
W Virginis	13 20 15	− 2 47·8	T Capricorni ...	21 15 50	−15 37·9
V Virginis	13 22 1	− 2 35·5	W Cygni.........	21 31 49	+44 52·5
R Hydræ	13 23 36	−22 42·1	S Cephei	21 36 36	+78 7·2
S Virginis	13 27 9	− 6 37·1	T Pegasi	22 3 25	+11 59·5
Z Virginis	14 4 19	−12 46·4	δ Cephei	22 25 1	+57 50·5
S Boötis	*14 19 8*	*+54 19·2*	R Lacertæ	22 38 18	+41 47·1
V Boötis.........	*14 25 16*	*+20 21·5*	S Aquarii	22 51 6	−20 56·4

MAXIMA AND MINIMA OF VARIABLE STARS.

M, signifies maximum ; *m*, minimum.

JANUARY.

d	
1	R Lyræ, *m.*
3	R Sagittæ, *m.*
3	R Sagittarii, M.
5	R Libræ, M.
7	U Virginis, M.
8	R Leonis, *m.*
8	R UrsæMaj., M.
11	U Monocer., *m.*
12	T Urs. Maj., *m.*
14	T Herculis, M.
14	W Cygni, M.
14	R Scuti, *m.*
15	S Vulpec., *m.*
15	T Aquarii, M.
15	V Tauri, M.
16	R Lyræ, M.
17	W Scorpii, M.
19	T Cassiop., M.
19	T Can. Min., M.
22	V Cygni, M.
24	S Delphini, M.
25	R Camelop., M.
28	S Aurigæ, M.
29	U Hydræ, M.
29	R Virginis, *m.*
30	L^2 Puppis, *m.*
30	U Boötis, *m.*
30	U Monocer., M.
31	S Serpent., M.

FEBRUARY.

d	
2	R Crateris, M.
5	V Coronæ, *m.*
7	T Virginis, M.
7	R Vulpec., *m.*
7	R Sagittæ, *m.*
9	S Hydræ, M.
12	S Vulpec., M.
15	S Urs. Maj., *m.*
16	R Aquilæ, M.
16	R Lyræ, *m.*
17	R Boötis, *m.*
18	R Aurigæ, M.
18	W Herculis, M.
18	R Scuti, M.
21	R Tauri, M.
22	T Pegasi, M.
22	R Arietis, M.
23	R Leporis, M.
24	S Sagittarii, M.
26	U Monocer., *m.*
26	S Aquilæ, *m.*
28	R Ophiuchi, M.

MARCH.

d	
2	R Lyræ, M.
3	S Cephei, M.
5	W Cygni, *m.*
5	R Persei, M.
8	T Arietis, M.
12	R Urs. Min., M.
13	S Libræ, M.
13	R Ceti, M.
14	R Sagittæ, *m.*
14	R Herculis, M.
16	U Monocer., M.
18	S Herculis, *m.*
18	T Hydræ, M.
18	T Libræ, M.
19	χ Cygni, M.
20	S Ophiuchi, M.
21	R Carini, *m.*
22	S Vulpec., *m.*
23	S Scorpii, M.
25	R Scuti, *m.*
28	R Delphini, M.
31	S Piscium, M.

APRIL.

d	
1	T Herculis, *m.*
2	R Lyræ, *m.*
2	R Lyncis, M.
4	T Capric., M.
5	R Can. Min., *m.*
5	S Can. Min., *m.*
6	L^2 Puppis, M.
7	R Lacertæ, M
7	R Virginis, M.
12	U Monocer., *m.*
13	R Vulpec., M.
16	T Cephei, M.
17	R Lyræ, M.
17	R Aquarii, M.
18	R Sagittæ, *m.*
20	S Vulpec., M.
23	S Aquarii, M.
23	U Virgin., *m.*
25	S Coronæ, M.
26	U Capric., M.
27	V Boötis, M.
29	U Boötis, M.
29	R Scuti, M.
29	T Geminor., M.
30	R Crateris, *m.*

MAY.

d	
1	U Monocer., M.
1	T Delphini, M.
2	T Urs. Maj., M.
7	T Aquarii, *m.*
10	R Androm., M.
14	R Draconis, *m.*
15	U Hydræ, *m.*
18	W Cygni, M.
18	R Lyræ, *m.*
22	S Persei, *m.*
23	R Sagittæ, *m.*
27	S Leonis, M.
28	U Monocer., *m.*
28	U Cancri, M.

Maxima and Minima of Variable Stars (continued).

MAY.

d
29 S Pegasi, M.
29 R Arietis, *m.*
29 S Vulpec., *m.*
29 R Boötis, M.
30 R Scorpii, M.

JUNE.

2 S Libræ, *m.*
2 R Lyræ, M.
2 R Corvi, M.
3 U Herculis, *m.*
4 R Scuti, *m.*
4 R Capric., M.
4 R Leonis, M.
8 V Virginis, M.
9 *o* Ceti, *m.*
9 S Urs. Maj., M.
11 V Geminor., M.
14 L^2 Puppis, *m.*
14 S Geminor., M.
15 *η* Geminor., M.
16 R Vulpec., *m.*
16 U Monocer., M.
16 S Cassiop., M.
23 R Virgin., *m.*
26 S Vulpec., M.
27 T Herculis, M.
27 R Sagittæ, *m.*
27 R Geminor., M.

JULY.

1 S Cygni, M.
2 V Tauri, M.
3 R Lyræ, M.
3 R Leo. Min., M.
4 R Sculpt., M.
6 T Cancri, *m.*
6 V Cancri, M.
7 R Urs. Min., *m.*
7 R Orionis, M.
7 R Cassiop., M.

JULY.

d
8 W Cygni, *m.*
10 R Scuti, M.
11 R Crateris, M.
13 U Monocer., *m.*
18 R Lyræ, *m.*
18 S Delphini, *m.*
19 R Cygni, M.
20 T Serpent., M.
20 W Herculis, *m.*
21 S Aquilæ, *m.*
22 U Boötis, *m.*
22 R Piscium, M.
26 V Coronæ, M.
28 R Pegasi, M.
29 T Cassiop., *m.*
31 U Can. Min., *m.*

AUGUST.

1 U Monocer., M.
1 R Hydræ, *m.*
1 U Virgin., M.
2 R Sagittæ, *m.*
4 S Vulpec., *m.*
5 T Aquarii, M.
11 U Hydræ, M.
12 T Sagittarii, M.
12 R Comæ, M.
15 R Scuti, *m.*
18 S Virgin., *m.*
18 R Lyræ, *m.*
20 L^2 Puppis, M.
20 U Cygni, *m.*
21 R Can. Min., M.
21 R Vulpec., M.
23 S Herculis, M.
26 R Arietis, M.
27 R Ceti, M.
27 S Boötis, M.
27 U Monocer., *m.*
28 W Ophiuchi, M.
28 W Scorpii, M.
29 R Aquilæ, *m.*

AUGUST.

d
30 R Draconis, M.
31 R Virgin., M.

SEPTEMBER.

2 S Vulpec., M.
2 R Carini, M.
2 R Lyræ, M.
6 R Sagittæ, *m.*
6 V Ophiuchi, M.
9 S Cephei, *m.*
9 Z Virgin., M.
13 T Herculis, *m.*
15 U Monocer., M.
16 S Scorpii, M.
18 R Leporis, *m.*
19 R Scuti, M.
19 S Can. Min., M.
20 W Cygni, M.
21 T Arietis, *m.*
22 S Libræ, M.
22 V Boötis, *m.*
27 R Boötis, *m.*
28 *o* Ceti, M.
28 T Urs. Maj., *m.*
29 R Sagittarii, M.
30 R Persei, M.

OCTOBER.

1 S Urs. Maj., *m.*
2 R Cancri, M.
3 R Lyræ, *m.*
4 S Arietis, M.
7 R Aurigæ, *m.*
7 R Crateris, *m.*
9 *χ* Cygni, *m.*
11 S Vulpec., *m.*
11 S Sagittarii, M.
11 R Sagittæ, *m.*
12 U Monocer., *m.*
17 R Camelop., M.

Maxima and Minima of Variable Stars (continued).

d	OCTOBER.	d	NOVEMBER.	d	DECEMBER.
18	R Lyræ, M.	7	R Ursæ Maj., M.	1	R Arietis, *m.*
18	T Ophiuchi, M.	8	S Vulpec., M.	3	R Lyræ, M.
22	U Boötis, M.	9	S Ophiuchi, M.	4	W Herculis, M.
23	T Cephei, *m.*	10	W Cygni, *m.*	8	U Herculis, M.
23	S Hydræ, M.	15	R Sagittæ, *m.*	9	T Herculis, M.
23	R Vulpec., *m.*	15	U Capric., M.	9	T Can. Min., M.
24	S Aurigæ, M.	15	R Virginis, *m.*	12	S Libræ, *m.*
25	R Scuti, *m.*	16	R Leonis, *m.*	13	R Serpent., M.
26	S Delphini, M.	16	U Virginis, *m.*	15	S Aquilæ, *m.*
28	L^2 Puppis, *m.*	18	R Lyræ, *m.*	16	U Monocer., M.
31	U Monocer., M.	19	S Ceti, M.	17	S Virgin., M.
		22	S Coronæ, *m.*	17	V Tauri, M.
		26	U Hydræ, *m.*	17	S Vulpec., *m.*
		26	T Orionis, M.	18	R Crateris, M.
		27	T Aquarii, *m.*	21	R Sagittæ, *m.*
		27	U Monocer., *m.*	27	R Ophiuchi, M.
		29	R Scuti, M.	29	T Capricor., M.
		30	S Leonis, M.	29	R Vulpec., M.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

U Cephei.	d	h	m
January.	2	22	53
	5	10	42
	7	22	32
	10	10	22
	12	22	12
	15	10	1
	17	21	51
	20	9	41
	22	21	31
	25	9	20
	27	21	10
	30	9	0
February.	1	20	49
	4	8	39
	6	20	29
	9	8	19
	11	20	8
	14	7	58
	16	19	48
	19	7	38
	21	19	27
	24	7	17
	26	19	7
	29	6	56
March.	2	18	46
	5	6	36
	7	18	26
	10	6	15
	12	18	5
	15	5	55
	17	17	45
	20	5	34
	22	17	24
	25	5	14
	27	17	3
	30	4	53
April.	1	16	43
	4	4	33
	6	16	22
	9	4	12
	11	16	2
	14	3	51
	16	15	41
	19	3	31
	21	15	21
	24	3	10
	26	15	0
	29	2	50

U Cephei.	d	h	m
May.	1	14	40
	4	2	29
	6	14	19
	9	2	9
	11	13	58
	14	1	48
	16	13	38
	19	1	28
	21	13	17
	24	1	7
	26	12	57
	29	0	47
	31	12	36
June.	3	0	26
	5	12	16
	8	0	5
	10	11	55
	12	23	45
	15	11	35
	17	23	24
	20	11	14
	22	23	4
	25	10	54
	27	22	43
	30	10	33
July.	2	22	23
	5	10	12
	7	22	2
	10	9	52
	12	21	42
	15	9	31
	17	21	21
	20	9	11
	22	21	0
	25	8	50
	27	20	40
	30	8	30
August.	1	20	19
	4	8	9
	6	19	59
	9	7	49
	11	19	38
	14	7	28
	16	19	18
	19	7	7
	21	18	57
	24	6	47
	26	18	37
	29	6	26
	31	18	16

U Cephei.	d	h	m
September.	3	6	6
	5	17	56
	8	5	45
	10	17	35
	13	5	25
	15	17	14
	18	5	4
	20	16	54
	23	4	44
	25	16	33
	28	4	23
	30	16	13
October.	3	4	3
	5	15	52
	8	3	42
	10	15	32
	13	3	21
	15	15	11
	18	3	1
	20	14	51
	23	2	40
	25	14	30
	28	2	20
	30	14	9
November.	2	1	59
	4	13	49
	7	1	39
	9	13	28
	12	1	18
	14	13	8
	17	0	58
	19	12	47
	22	0	37
	24	12	27
	27	0	16
	29	12	6
December.	1	23	56
	4	11	46
	6	23	35
	9	11	25
	11	23	15
	14	11	5
	16	22	54
	19	10	44
	21	22	34
	24	10	23
	26	22	13
	29	10	3
	31	21	53

Algol.	d	h	m
January.	3	6	55
	6	3	43
	9	0	32
	11	21	21
	14	18	10
	17	14	59
	20	11	48
	23	8	37
	26	5	26
	29	2	15
	31	23	4
February.	3	19	52
	6	16	41
	9	13	30
	12	10	19
	15	7	8
	18	3	57
	21	0	46
	23	21	35
	26	18	24
	29	15	12
March.	3	12	1
	6	8	50
	9	5	39
	12	2	28
	14	23	17
	17	20	6
	20	16	55
	23	13	44
	26	10	33
	29	7	21
April.	1	4	10
	4	0	59
	6	21	48
	9	18	37
	12	15	26
	15	12	15
	18	9	4
	21	5	53
	24	2	41
July.	1	22	15
	4	19	4
	7	15	53
	10	12	42
	13	9	31
	16	6	19

Algol.	d	h	m
July.	19	3	8
	21	23	57
	24	20	46
	27	17	35
	30	14	24
August.	2	11	13
	5	8	2
	8	4	51
	11	1	39
	13	22	28
	16	19	17
	19	16	6
	22	12	55
	25	9	44
	28	6	33
	31	3	22
September.	3	0	11
	5	21	0
	8	17	48
	11	14	37
	14	11	26
	17	8	15
	20	5	4
	23	1	53
	25	22	42
	28	19	31
October.	1	16	20
	4	13	8
	7	9	57
	10	6	46
	13	3	35
	16	0	24
	18	21	13
	21	18	2
	24	14	51
	27	11	40
	30	8	29
November.	2	5	17
	5	2	6
	7	22	55
	10	19	44
	13	16	33
	16	13	22
	19	10	11
	22	7	0
	25	3	49
	28	0	37
	30	21	26

Minima of Variable Stars of the Algol Type (continued).

ALGOL.

	d	h	m
December.	3	18	15
	6	15	4
	9	11	53
	12	8	42
	15	5	31
	18	2	20
	20	23	9
	23	19	58
	26	16	46
	29	13	35

λ TAURI.

	d	h	m
January.	3	6	23
	7	5	15
	11	4	8
	15	3	0
	19	1	52
	23	0	45
	26	23	37
	30	22	29
February.	3	21	22
	7	20	14
	11	19	6
	15	17	58
	19	16	51
	23	15	43
	27	14	35
March.	2	13	28
	6	12	20
	10	11	12
	14	10	5
	18	8	57
July.	14	23	6
	18	21	58
	22	20	50
	26	19	43
	30	18	35
August.	3	17	27
	7	16	20
	11	15	12
	15	14	4
	19	12	57
	23	11	49
	27	10	41
	31	9	33
September.	4	8	26
	8	7	18
	12	6	10
	16	5	3
	20	3	55
	24	2	47
	28	1	40
October.	2	0	32
	5	23	24
	9	22	16
	13	21	9
	17	20	1
	21	18	53
	25	17	46
	29	16	38
November.	2	15	30
	6	14	23
	10	13	15
	14	12	7
	18	10	59
	22	9	52
	26	8	44
	30	7	36
December.	4	6	29
	8	5	21
	12	4	13
	16	3	6
	20	1	58
	24	0	50
	27	23	42
	31	22	35

S CANCRI.

	d	h	m
Jan.	9	11	12
	18	22	50
	28	10	28
Feb.	6	22	5
	16	9	43
	25	21	21
Mar.	6	8	59
	15	20	36
	25	8	14
April.	3	19	52
	13	7	30
	22	19	7
May.	2	6	45
	11	18	23
	21	6	1
	30	17	38
June.	9	5	16
	18	16	54
	28	4	32
July.	7	16	9
Sept.	12	1	34
	21	13	11
Oct.	1	0	49
	10	12	27
	20	0	5
	29	11	42
Nov.	7	23	20
	17	10	58
	26	22	36
Dec.	6	10	13
	15	21	51
	25	9	29

δ LIBRÆ.

	d	h	m
January.	1	9	9
	3	17	0
	6	0	52
	8	8	43
	10	16	34
	13	0	26
	15	8	17
	17	16	8
	20	0	0
	22	7	51
	24	15	42
	26	23	34
	29	7	25
	31	15	16
February.	2	23	8
	5	6	59
	7	14	50
	9	22	42
	12	6	33
	14	14	24
	16	22	16
	19	6	7
	21	13	58
	23	21	50
	26	5	41
	28	13	32
March.	1	21	24
	4	5	15
	6	13	6
	8	20	58
	11	4	49
	13	12	40
	15	20	32
	18	4	23
	20	12	14
	22	20	6
	25	3	57
	27	11	48
	29	19	40
April.	1	3	31
	3	11	22
	5	19	4
	8	3	5
	10	10	56
	12	18	48
	15	2	39
	17	10	30
	19	18	22
	22	2	13
	24	10	4
	26	17	56
	29	1	47
May.	1	9	38
	3	17	30
	6	1	21
	8	9	12
	10	17	4
	13	0	55
	15	8	46
	17	16	38
	20	0	29
	22	8	20
	24	16	12
	27	0	3
	29	7	54
	31	15	46
June.	2	23	37
	5	7	28
	7	15	20
	9	23	11
	12	7	2
	14	14	54
	16	22	45
	19	6	36
	21	14	28
	23	22	19
	26	6	10
	28	14	2
	30	21	53
July.	3	5	44
	5	13	36
	7	21	27
	10	5	18
	12	13	10
	14	21	1
	17	4	52
	19	12	44
	21	20	35
	24	4	26
	26	12	18
	28	20	9
	31	4	0
August.	2	11	52
	4	19	43
	7	3	34
	9	11	26
	11	19	17
	14	3	8
	16	11	0
	18	18	51
	21	2	42
	23	10	34
	25	18	25
	28	2	16
	30	10	8
September.	1	17	59
	4	1	50
	6	9	42
	8	17	33
	11	1	24
	13	9	16
	15	17	7
	18	0	58
	20	8	50
	22	16	41
	25	0	32
	27	8	24
	29	16	15
Oct.	2	0	6
	4	7	58
	6	15	49
December.	1	12	21
	3	20	12
	6	4	4
	8	11	55
	10	9	46
	13	3	38
	15	11	29
	17	19	20
	20	3	12
	22	11	3
	24	18	54
	27	2	46
	29	10	37
	31	18	28

Minima of Variable Stars of the Algol Type (continued).

U CORONÆ.	d	h	m
January.	4	1	54
	7	12	45
	10	23	36
	14	10	28
	17	21	19
	21	8	10
	24	19	1
	28	5	53
	31	16	44
February.	4	3	35
	7	14	26
	11	1	18
	14	12	9
	17	23	0
	21	9	51
	24	20	42
	28	7	34
March.	2	18	25
	6	5	16
	9	16	7
	13	2	59
	16	13	50
	20	0	41
	23	11	32
	26	22	24
	30	9	15
April.	2	20	6
	6	6	57
	9	17	49
	13	4	40
	16	15	31
	20	2	22
	23	13	14
	27	0	5
	30	10	56
May.	3	21	47
	7	8	39
	10	19	30
	14	6	21
	17	17	12
	21	4	4
	24	14	55
	28	1	46
	31	12	37
June.	3	23	29
	7	10	20
	10	21	11
	14	8	2

U CORONÆ.	d	h	m
June.	17	18	54
	21	5	45
	24	16	36
	28	3	27
July.	1	14	18
	5	1	10
	8	12	1
	11	22	52
	15	9	43
	18	20	35
	22	7	26
	25	18	17
	29	5	8
August.	1	16	0
	5	2	51
	8	13	42
	12	0	33
	15	11	25
	18	22	16
	22	9	7
	25	19	58
	29	6	50
September.	1	17	41
	5	4	32
	8	15	23
	12	2	15
	15	13	6
	18	23	57
	22	10	48
	25	21	40
	29	8	31
October.	2	19	22
	6	6	13
	9	17	5
	13	3	56
	16	14	47
	20	1	38
	23	12	30
	26	23	21
	30	10	12
November.	2	21	3
	6	7	54
	9	18	46
	13	5	37
	16	16	28
	20	3	19
	23	14	11
	27	1	2
	30	11	53

U CORONÆ.	d	h	m
December.	3	22	44
	7	9	36
	10	20	27
	14	7	18
	17	18	9
	21	5	1
	24	15	52
	28	2	43
	31	13	34

U OPHIUCHI.	d	h	m
January.	1	19	44
	4	8	7
	6	20	30
	9	8	53
	11	21	16
	14	9	39
	16	22	2
	19	10	25
	21	22	48
	24	11	11
	26	23	34
	29	11	57
February.	1	0	20
	3	12	43
	6	1	6
	8	13	29
	11	1	52
	13	14	16
	16	2	39
	18	15	2
	21	3	25
	23	15	48
	26	4	11
	28	16	34
March.	2	4	57
	4	17	20
	7	5	43
	9	18	6
	12	6	29
	14	18	52
	17	7	15
	19	19	38
	22	8	1
	24	20	24
	27	8	47
	29	21	10

U OPHIUCHI.	d	h	m
April.	1	9	33
	3	21	56
	6	10	19
	8	22	42
	11	11	5
	13	23	28
	16	11	52
	19	0	15
	21	12	38
	24	1	1
	26	13	24
	29	1	47
May.	1	14	10
	4	2	33
	6	14	56
	9	3	19
	11	15	42
	14	4	5
	16	16	28
	19	4	51
	21	17	14
	24	5	37
	26	18	0
	29	6	23
	31	18	46
June.	3	7	9
	5	19	32
	8	7	55
	10	20	18
	13	8	41
	15	21	4
	18	9	28
	20	21	51
	23	10	14
	25	22	37
	28	11	0
	30	23	23
July.	3	11	46
	6	0	9
	8	12	32
	11	0	55
	13	13	18
	16	1	41
	18	14	4
	21	2	27
	23	14	50
	26	3	13
	28	15	36
	31	3	59

U OPHIUCHI.	d	h	m
August.	2	16	22
	5	4	45
	7	17	8
	10	5	31
	12	17	54
	15	6	17
	17	18	40
	20	7	4
	22	19	27
	25	7	50
	27	20	13
	30	8	36
September.	1	20	59
	4	9	22
	6	21	45
	9	10	8
	11	22	31
	14	10	54
	16	23	17
	19	11	40
	22	0	3
	24	12	26
	27	0	49
	29	13	12
October.	2	1	35
	4	13	58
	7	2	21
	9	14	44
	12	3	7
	14	15	30
	17	3	53
	19	16	16
	22	4	40
	24	17	3
	27	5	26
	29	17	49
November.	1	6	12
	3	18	35
	6	6	58
	8	19	21
	11	7	44
	13	20	7
	16	8	30
	18	20	53
	21	9	16
	23	21	39
	26	10	2
	28	22	25

Note.—For U Ophiuchi, the period of which is $20^h\ 8^m$, only every third minimum is given.

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Minima of Variable Stars of the Algol Type (continued).

See Jan Obsy

Y

[illegible] Cygni.

	d	h	m
January.	3	2	24
	6	2	20
	9	2	16
	12	2	12
	15	2	8
	18	2	4
	21	2	0
	24	1	56
	27	1	52
	30	1	48
February.	2	1	44
	5	1	40
	8	1	36
	11	1	32
	14	1	28
	17	1	24
	20	1	20
	23	1	16
	26	1	12
	29	1	8
Mar.	3	1	4
	6	1	0
	9	0	56

[illegible] Cygni.

	d	h	m
March.	12	0	52
	15	0	48
	18	0	44
	21	0	40
	24	0	36
	27	0	32
	30	0	28
April.	2	0	24
	5	0	20
	8	0	16
	11	0	12
	14	0	8
	17	0	4
	20	0	0
	22	23	56
	25	23	52
	28	23	48
May.	1	23	44
	4	23	40
	7	23	36
	10	23	32
	13	23	28
	16	23	24

[illegible] Cygni.

	d	h	m
May.	19	23	20
	22	23	16
	25	23	12
	28	23	8
	31	23	4
June.	3	23	0
	6	22	56
	9	22	52
	12	22	48
	15	22	44
	18	22	40
	21	22	36
	24	22	32
	27	22	28
	30	22	24
July.	3	22	20
	6	22	16
	9	22	12
	12	22	8
	15	22	4
	18	22	0
	21	21	56
	24	21	52
	27	21	48
	30	21	44

[illegible] Cygni.

	d	h	m
August.	2	21	40
	5	21	36
	8	21	32
	11	21	28
	14	21	24
	17	21	20
	20	21	16
	23	21	12
	26	21	8
	29	21	4
September.	1	21	0
	4	20	56
	7	20	52
	10	20	48
	13	20	44
	16	20	40
	19	20	36
	22	20	32
	25	20	28
	28	20	24
Oct.	1	20	20
	4	20	16
	7	20	12
	10	20	8

[illegible] Cygni.

	d	h	m
October.	13	20	4
	16	20	0
	19	19	56
	22	19	52
	25	19	48
	28	19	44
	31	19	40
November.	3	19	36
	6	19	32
	9	19	28
	12	19	24
	15	19	20
	18	19	16
	21	19	12
	24	19	8
	27	19	4
	30	19	0
December.	3	18	56
	6	18	52
	9	18	48
	12	18	44
	15	18	40
	18	18	36
	21	18	32
	24	18	28
	27	18	24
	30	18	20

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

Note.—The times of minima only are given; the times of maxima may be found by adding the interval printed under the name of the star.

T Monocerotis.

($7^d\ 22^h$.)

	d	h		d	h
Jan.	13	20	July	20	18
Feb.	9	20	Aug.	16	18
Mar.	7	19	Sept.	12	17
Apr.	3	19	Oct.	9	17
	30	19	Nov.	5	17
May	27	19	Dec.	2	17
June	23	18		29	16

ζ Geminorum.

($5^d\ 0^h$.)

	d	h		d	h
Jan.	8	15	Feb.	8	3
	18	19		18	6
	28	23		28	10

ζ Geminorum (*continued*).

	d	h		d	h
Mar.	9	14	Sept.	18	12
	19	17		28	16
	29	21	Oct.	8	20
Apr.	9	1		19	0
	19	5		29	3
	29	8	Nov.	8	7
May	9	12		18	11
	19	16		28	14
	29	19	Dec.	8	18
Aug.	19	1		18	22
	29	5		29	2
Sept.	8	9			

W Virginis.

($8^d\ 5^h$.)

	d	h		d	h
Jan.	8	10	June	11	21
	25	17		29	4
Feb.	12	0	July	16	10
	29	6	Aug.	2	17
Mar.	17	13		19	23
Apr.	3	19	Oct.	28	2
	21	2	Nov.	14	8
May	8	8	Dec.	1	15
	25	15		18	21

X (3) Sagittarii.

($2^d\ 21^h$.)

	d	h		d	h
Feb.	1	17	Feb.	15	17
	8	17		22	17

VARIABLES OF SHORT PERIOD (*continued*).

X (3) SAGITTARII (*continued*).

	d	h		d	h
Feb.	29	18	July	18	23
Mar.	7	18		26	0
	14	18	Aug.	2	0
	21	18		9	0
	28	19		16	0
Apr.	4	19		23	1
	11	19		30	1
	18	20	Sept.	6	1
	25	20		13	2
May	2	20		20	2
	9	20		27	2
	16	21	Oct.	4	2
	23	21		11	3
	30	21		18	3
June	6	22		25	3
	13	22	Nov.	1	4
	20	22		8	4
	27	22		15	4
July	4	23		22	4
	11	23		29	5

W (γ') SAGITTARII.
(3^d 4^h.)

	d	h		d	h
Feb.	7	22	July	8	19
	15	12		16	9
	23	2		23	23
Mar.	1	17		31	13
	9	7	Aug.	8	4
	16	21		15	18
	24	11		23	8
Apr.	1	2		30	22
	8	16	Sept.	7	13
	16	6		15	3
	23	20		22	17
May	1	11		30	7
	9	1	Oct.	7	22
	16	15		15	12
	24	5		23	2
	31	20		30	16
June	8	10	Nov.	7	7
	16	0		14	21
	23	14		22	11
July	1	4		30	1

Z SAGITTARII.
(1^d 19^h.)

	d	h		d	h
Feb.	5	12	Feb.	22	18
	11	6		28	12
	17	0	Mar.	5	6

Z SAGITTARII (*continued*).

	d	h		d	h
Mar.	11	0	July	27	0
	16	18	Aug.	1	18
	22	12		7	12
	28	6		13	6
Apr.	3	0		19	0
	8	18		24	18
	14	12		30	12
	20	6	Sept.	5	6
	26	0		11	0
May	1	18		16	18
	7	12		22	12
	13	6		28	6
	19	0	Oct.	4	0
	24	18		9	18
	30	12		15	12
June	5	6		21	6
	11	0		27	0
	16	18	Nov.	1	18
	22	12		7	12
	28	6		13	6
July	4	0		19	0
	9	18		24	18
	15	12		30	12
	21	6			

U SAGITTARII.
(2^d 23^h.)

	d	h		d	h
Feb.	2	4	July	6	7
	8	22		13	1
	15	16		19	19
	22	10		26	13
	29	4	Aug.	2	7
Mar.	6	22		9	1
	13	15		15	19
	20	9		22	13
	27	3		29	7
Apr.	2	21	Sept.	5	0
	9	15		11	18
	16	9		18	12
	23	3		25	6
	29	21	Oct.	2	0
May	6	14		8	18
	13	8		15	12
	20	2		22	6
	26	20		28	23
June	2	14	Nov.	4	17
	9	8		11	11
	16	2		18	5
	22	20		24	23
	29	14			

β LYRÆ.
(Max. 3^d 5^h.
Secondary Min. 6^d 11^h.
Max. 9^d 16^h.)

	d	h		d	h
Jan.	12	13	July	11	8
	25	11		24	6
Feb.	7	9	Aug.	6	4
	20	7		19	2
Mar.	4	5	Sept.	1	0
	17	3		13	22
	30	1		26	20
Apr.	11	23	Oct.	9	18
	24	21		22	16
May	7	19	Nov.	4	14
	20	17		17	12
June	2	15		30	10
	15	13	Dec.	13	8
	28	10		26	6

U AQUILÆ
(2^d 12^h.)

	d	h		d	h
Feb.	3	17	July	13	17
	10	17		20	17
	17	17		27	17
	24	17	Aug.	3	17
Mar.	2	17		10	17
	9	17		17	17
	16	17		24	17
	23	17		31	17
	30	17	Sept.	7	17
Apr.	6	17		14	17
	13	17		21	17
	20	17		28	17
	27	17	Oct.	5	17
May	4	17		12	17
	11	17		19	17
	18	17		26	17
	25	17	Nov.	2	17
June	1	17		9	17
	8	17		16	17
	15	17		23	17
	22	17		30	17
	29	17	Dec.	7	17
July	6	17		14	17

η AQUILÆ.
(2^d 9^h.)

	d	h		d	h
Feb.	1	20	Feb.	23	9
	9	0	Mar.	1	13
	16	4		8	17

VARIABLES OF SHORT PERIOD (*continued*).

η AQUILÆ
(*continued.*)

	d	h		h	d
Mar.	15	21	Aug.	13	14
	23	2		20	18
	30	6		27	23
Apr.	6	10	Sept.	4	3
	13	14		11	7
	20	19		18	11
	27	23		25	16
May	5	3	Oct.	2	20
	12	7		10	0
	19	11		17	4
	26	16		24	9
June	2	20		31	13
	10	0	Nov.	7	17
	17	4		14	21
	24	9		22	2
July	1	13		29	6
	8	17	Dec.	6	10
	15	21		13	14
	23	2		20	18
	30	6		27	23
Aug.	6	10			

S (10) SAGITTÆ.
(3d 0h.)

	d	h		d	h
Feb.	5	16	July	22	8
	14	1		30	17
	22	10	Aug.	8	2
Mar.	1	20		16	11
	10	5		24	20
	18	14	Sept.	2	6
	26	23		10	15
Apr.	4	8		19	0
	12	17		27	9
	21	3	Oct.	5	18
	29	12		14	4
May	7	21		22	13
	16	6		30	22
	24	15	Nov.	8	7
June	2	1		16	16
	10	10		25	1
	18	19	Dec.	3	11
	27	4		11	20
July	5	13		20	5
	13	23		28	14

X CYGNI.
(4d 0h.)

	h	d		h	d
Jan.	5	22	July	6	11
	19	23		20	12
Feb.	3	0	Aug.	3	13
	17	1		17	14
Mar.	2	2		31	15
	16	3	Sept.	14	16
	30	4		28	17
Apr.	13	5	Oct.	12	18
	27	6		26	19
May	11	7	Nov.	9	20
	25	8		23	21
June	8	9	Dec.	7	22
	22	10		21	23

T VULPECULÆ.
(3d 9h.)

	d	h		h	d
Jan.	2	16	May	19	5
	7	3		23	16
	11	13		28	2
	16	0	June	1	13
	20	10		5	23
	24	21		10	10
	29	7		14	20
Feb.	2	18		19	7
	7	4		23	17
	11	15		28	4
	16	1	July	2	14
	20	12		7	1
	24	22		11	11
	29	8		15	21
Mar.	4	19		20	8
	9	5		24	18
	13	16		29	5
	18	2	Aug.	2	15
	22	13		7	2
	26	23		11	12
	31	10		15	23
Apr.	4	20		20	9
	9	7		24	20
	13	17		29	6
	18	4	Sept.	2	17
	22	14		7	3
	27	1		11	14
May	1	11		16	0
	5	22		20	11
	10	8		24	21
	14	19		29	8

T VULPECULÆ
(*continued.*).

	h	d		h	d
Oct.	3	18	Nov.	21	13
	8	5		26	0
	12	15		30	10
	17	2	Dec.	4	21
	21	12		9	7
	25	23		13	18
	30	9		18	4
Nov.	3	20		22	15
	8	6		27	1
	12	17		31	12
	17	3			

δ CEPHEI.
(1d 15h.)

	d	h		d	h
Jan.	6	1	July	6	1
	11	10		11	21
	16	18		17	6
	22	3		22	14
	27	12		27	23
Feb.	1	21	Aug.	2	8
	7	6		7	17
	12	14		13	1
	17	23		18	10
	23	8		23	19
	28	17		29	4
Mar.	5	2	Sept.	3	13
	10	10		8	21
	15	19		14	6
	21	4		19	15
	26	13		25	0
	31	22		30	9
Apr.	6	6	Oct.	5	17
	11	15		11	2
	17	0		16	11
	22	9		21	20
	27	18		27	5
May	3	2	Nov.	1	13
	8	11		6	22
	13	20		12	7
	19	5		17	16
	24	14		23	1
	29	22		28	9
June	4	7	Dec.	3	18
	9	16		9	3
	15	1		14	12
	20	10		19	21
	25	18		25	5
July	1	3		30	14

BINARY STARS.

The following are amongst the best known and most remarkable.

No.	Name of Star.	R.A. 1890.	Dec. 1890.	Mag.	Position.	Distance.	Epoch. 1800+.	Observer.
		h m	° ′		°	″		
1.	η Cassiopeiæ	0 42·4	+57 14	4; 8	165·7	5·16	82·87	E.
2.	36 Andromedæ	0 49·1	+23 2	6; 7	2·0	1·35	85·28	P.
3.	*p* Eridani	1 35·6	−56 45	6; 6	230·5	7·10	85·19	T.
4.	ι Cassiopeiæ AB	2 20·0	+66 55	4; 7	176·3	4·93	86·07	E.
5.	„ AC	„	„	4; 8	110·0	7·43	83·52	E.
6.	γ Ceti	2 37·6	+ 2 46	3; 7	289·7	2·94	83·95	P.
7.	98 Eridani	3 31·1	+ 0 14	6; 8	241·9	6·47	80·02	H.
8.	o² „ AB	4 10·2	− 7 49	4; 9	107·1	81·01	80·15	β.
9.	2 Camelopardi	4 31·2	+53 16	5; 7	291·9	1·87	82·93	E.
10.	*i* Orionis OΣ 98	5 1·9	+ 8 21	6; 7	202·2	0·99	84·21	E.
11.	118 Tauri	5 22·5	+25 4	6; 7	199·4	5·10	82·18	S.
12.	12 Lyncis AB	6 36·5	+59 33	5; 6	128·4	1·70	82·76	E.
13.	„ AC	„	„	5; 7	126·7	8·29	82·20	E.
14.	Sirius	6 40·3	−16 34	1; 9	30·6	7·39	86·22	H.
15.	*e* Geminorum	6 48·4	+13 19	5; 8	163·4	6·19	82·89	E.
16.	Castor AB	7 27·6	+32 8	2; 3	232·2	5·75	86·33	E.
17.	ζ Cancri AB	8 5·9	+17 59	6; 7	56·4	1·08	86·30	E.
18.	„ $\left.\frac{A+BC}{2}\right\}$	„	„	6; 7	127·2	5·35	86·30	E.
19.	ε Hydræ	8 40·9	+ 6 49	4; 8	226·0	3·43	82·89	E.
20.	ι Ursæ Majoris	8 51·7	+48 29	4; 12	356·7	9·56	83·40	E.
21.	σ² Ursæ Majoris	9 0·7	+67 35	5; 8	238·7	2·65	82·86	E.
22.	ω Leonis	9 22·6	+ 9 32	6; 7	92·2	0·73	86·32	E.
23.	γ Leonis AB	10 13·9	+20 24	2; 4	113·0	3·45	85·12	E.
24.	ξ Ursæ Majoris	11 12·3	+32 9	4; 5	237·4	2·06	86·36	E.
25.	ι Leonis	11 18·2	+11 8	4; 8	64·0	2·58	84·20	P.
26.	γ Centauri	12 35·5	−48 21	4; 4	1·3	1·39	80·46	R.
27.	γ Virginis	12 36·1	− 0 51	3; 3	336·1	5·32	84·89	E.
28.	35 Comæ	12 47·9	+21 51	5; 8	65·4	1·16	85·34	P.
39.	α Comæ	13 4·7	+18 7	6; 6	192·1	0·56	82·93	E.
30.	Σ 1757	13 28·7	+ 0 15	8; 9	69·4	2·31	82·47	S.
31.	25 Canum Venat.	13 32·6	+36 52	6; 7	329·1	0·89	85·37	P.
32.	ο Virginis	13 37·6	+ 4 6	6; 8	230·1	3·45	79·41	H.
33.	Σ 1819	14 9·7	+ 3 40	8; 8	11·5	1·39	85·41	P.
34.	α Centauri	14 32·1	−60 23	1; 2	200·8	14·05	85·57	T.
35.	ε Boötis	14 40·2	+27 32	3; 6	328·0	2·88	85·41	P.
36.	ξ Boötis	14 46·3	+19 34	5; 7	259·4	3·32	86·60	E.
37.	44 Boötis	15 0·2	+48 5	5; 6	241·1	5·00	85·62	J.
38.	η Coronæ Borealis	15 18·7	+30 41	5; 6	179·5	0·57	86·64	E.
39.	μ² Boötis	15 20·3	+37 46	7; 8	106·2	0·70	86·78	J.
40.	δ Serpentis	15 29·6	+10 55	3; 5	189·9	3·52	86·56	B.
41.	OΣ 298	15 31·7	+40 11	7; 8	104·9	0·29	86·68	E.
42.	ξ (51) Scorpii	15 58·3	−11 4	5; 5	194·5	1·14	83·60	Se.
43.	σ Coronæ Borealis	16 10·6	+34 8	9; 9	207·3	4·09	85·74	E.
44.	Antares	16 22·7	−26 11	1; 8	271·7	2·92	80·02	β.
45.	λ Ophiuchi	16 25·4	+ 2 14	4; 6	44·1	1·57	86·66	E.

BINARY STARS (*continued*).

No.	Name of Star.	R.A. 1890.	Dec. 1890.	Mag.	Position.	Distance.	Epoch 1800+.	Observer.
		h m	° ′		°	″		
46.	ζ Herculis	16 37·1	+31 48	3 ; 7	89·9	1·78	86·75	E.
47.	Σ 2107	16 47·5	+28 51	7 ; 9	236·1	0·78	85·57	E.
48.	Σ 2120	17 0·4	+28 15	6 ; 9	249·6	5·61	85·49	P.
49.	A Ophiuchi AB	17 8·6	−26 26	5 ; 7	230·1	4·51	77·00	H.
50.	ρ Herculis	17 19·9	+37 15	4 ; 5	311·6	3·88	83·66	J.
51.	Σ 2173	17 24·7	− 0 58	6 ; 6	21·9	0·30	85·66	E.
52.	τ Ophiuchi	17 57·1	− 8 11	5 ; 6	256·2	1·85	86·62	J.
53.	70 Ophiuchi	17 59·9	+ 2 32	4 ; 6	13·7	2·01	86·67	J.
54.	ε¹ Lyræ	18 40·7	+39 33	5 ; 6	15·7	3·35	86·83	J.
55.	ε² Lyræ	18 40·7	+39 29	5 ; 5	134·2	2·41	86·83	J.
56.	γ Coronæ Australis ...	18 59·0	−37 13	6 ; 6	37·8	1·62	83·62	W.
57.	δ Cygni	19 41·5	+44 52	3 ; 8	319·0	1·84	85·90	E.
58.	γ Delphini........... ..	20 41·6	+15 44	4 ; 5	271·1	11·01	86·73	J.
59.	Σ 2729	20 45·6	− 6 1	6 ; 7	155·9	0·40	79·76	H.
60.	1 Equulei AB	20 53·6	+ 3 54	6 ; 6	284·7	0·97	86·24	J.
61.	61 Cygni	21 2·0	+38 13	5 ; 6	118·9	20·45	83·85	Se.
62.	μ Cygni	21 39·2	+28 15	4 ; 5	119·5	3·18	85·67	B.
63.	ξ Cephei..................	22 0·6	+64 5	5 ; 6	284·0	6·53	85·69	B.
64.	ζ Aquarii	22 23·2	− 0 35	4 ; 4	326·8	3·18	75·77	B.
65.	o Cephei...........	23 14·1	+67 31	5 ; 8	191·3	2·83	83·36	E.

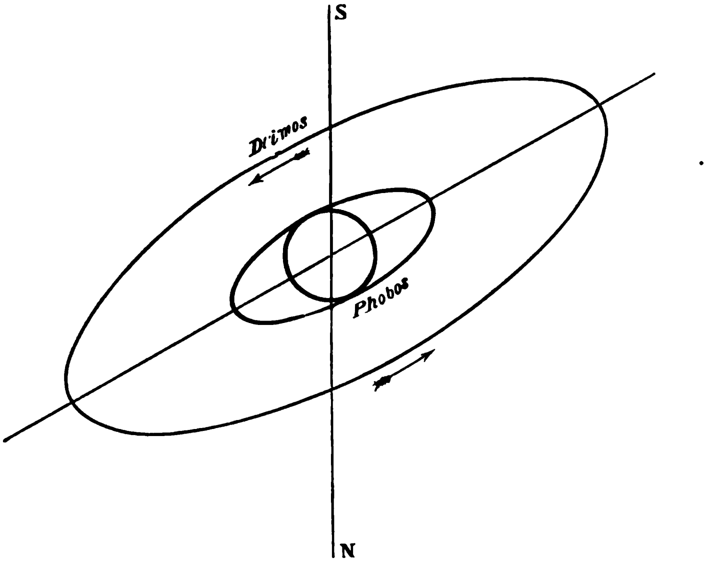
In the column headed "Observer," B. denotes Dr. L. de Ball ; E., Dr. R. Engelmann ; H., Prof. Asaph Hall ; P., Mons. J. Perrotin ; R., Mr. H. C. Russell ; S., Prof. Schiaparelli ; T., Mr. Tebbutt ; W., Mr. H. C. Wilson ; J., Dr. Jedrzejewicz ; β., Mr. Burnham ; Se., Mr. Seabroke.

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		

SELENOGRAPHICAL LONGITUDES AND LATITUDES OF LUNAR CRATERS.

Name of Crater.	Longitude. += West. −= East.	Latitude. += North. −= South.	Name of Crater.	Longitude. += West. −= East.	Latitude. += North. −= South.
	° ′	° ′		° ′	° ′
Schubert A	+77 16	+ 2 28	Alphonsus A ...	− 3 14	−12 59
Hansen A	+74 0	+13 17	Mösting	− 5 35	− 0 36
Langrenus	+60 34	− 8 22	Moretus	− 7 9	−69 45
Petavius	+59 16	−24 39	Archimedes A...	− 7 11	+27 45
Cleomedes A ...	+54 17	+28 24	Hell	− 8 20	−31 59
Picard	+53 52	+14 28	Lalande	− 8 47	− 4 25
Thales	+49 12	+61 58	Pico	− 9 13	+45 28
Messier	+47 9	− 1 59	Eratosthenes ...	−11 34	+14 25
Proclus	+46 30	+16 10	Tycho	−11 52	−42 52
Taruntius	+45 58	+ 5 40	Timocharis	−13 0	+26 43
Cepheus A	+45 40	+40 59	Clavius C.........	−14 40	−57 17
Goclenius	+44 27	− 9 59	Hesiodus B......	−17 0	−26 50
Fabricius	+40 46	−42 8	Copernicus	−19 56	+ 9 21
Hercules	+38 23	+46 23	Pytheas	−20 34	+20 14
Capella	+34 48	− 7 33	Bullialdus	−22 6	−20 26
Democritus......	+33 30	+62 8	Reinhold	−22 37	+ 3 13
Censorinus	+32 27	− 0 24	Carlini...........	−24 1	+33 23
Piccolomini ...	+31 35	−29 11	La Hire	−25 10	+27 18
Vitruvius.........	+31 2	+17 36	Laplace A	−26 34	+43 16
Maskelyne	+29 35	+ 2 32	Scheiner A	−26 36	−59 58
Mutus	+29 22	−63 6	Campanus	−27 27	−27 37
Mädler	+29 12	−10 56	Mayer	−28 50	+15 33
Posidonius A ...	+29 7	+31 36	Euler	−28 57	+22 57
LeMonnier A ...	+29 4	+26 0	Euclides	−29 16	− 7 10
Burg	+27 32	+44 57	Milichius.........	−29 40	+10 0
Theophilus	+26 18	−11 21	Landsberg A ...	−31 5	+ 0 2
Lindenau	+24 30	−31 52	Ramsden	−31 42	−32 26
Aristoteles C ...	+23 34	+57 26	Heraclides	−34 1	+41 8
Plinius	+23 23	+15 17	Delisle...........	−34 48	+29 59
Bessel	+17 22	+21 54	Brayley	−36 25	+20 54
Delambre	+17 15	− 1 47	Encke	−36 36	+ 4 18
Dionysius	+17 9	+ 2 51	Bessarion E......	−37 1	+14 59
Sacrobosco	+15 41	−23 42	Vitello	−37 8	−30 0
Menelaus....	+15 31	+16 24	Kepler...........	−37 41	+ 7 57
Dollond	+14 12	−10 15	Wichmann	−37 56	− 7 41
Linné	+11 32	+27 47	Gassendi	−39 31	−16 58
Agrippa	+10 18	+ 4 2	Flamsteed	−44 12	− 4 31
Manilius	+ 8 47	+14 27	Wollaston	−46 54	+30 17
Hipparchus C...	+ 8 4	− 7 23	Aristarchus......	−47 12	+23 17
Archytas.........	+ 4 13	+58 24	Drebbel	−48 13	−40 47
Cassini A	+ 4 9	+40 23	Marius	−49 57	−11 59
Albategnius ...	+ 3 58	−11 21	Billy	−49 58	−14 0
Werner	+ 2 58	−27 46	Pythagoras	−61 37	+63 4
Conon	+ 1 57	+21 31	Seleucus	−65 48	+20 54
Ukert	+ 1 9	+ 7 48	Crüger...........	−66 40	−16 46
Aristillus	+ 1 7	+33 45	Lichtenberg ...	−67 5	+31 25
Murchison A ...	+ 1 0	− 4 4	Eichstädt	−70 27	−20 31
Ptolemaus A ...	− 0 58	− 8 35	Harding	−70 52	+43 9
Herschel	− 2 9	− 5 37	Grimaldi A......	−70 53	− 4 54
Bode	− 2 38	+ 6 38	Olbers	−77 33	+ 7 55



Apparent Orbits of the Satellites cf Mars, as seen in an Inverting Telescope.

SATELLITES OF MARS.

East Elongations.

PHOBOS.

March.	March.	April.	April.	April.	April.	May.
d h	d h	d h	d h	d h	d h	d h
20 5·1	27 13·1	2 22·1	10 6·1	17 14·0	24 22·0	1 7·1
12·7	20·7	3 5·7	13·7	21·7	25 5·6	14·7
20·4	28 4·3	13·4	21·4	18 5·3	13·3	22·3
21 4·0	12·0	21·0	11 5·0	13·0	20·9	2 6·0
11·7	19·6	4 4·7	12·7	20·6	26 4·6	13·6
19·3	29 3·3	12·3	20·3	19 4·3	12·2	21·3
22 3·0	10·9	20·0	12 3·9	11·9	19·9	3 4·9
10·6	18·6	5 3·6	11·6	19·6	27 3·6	12·6
18·3	30 2·2	11·3	19·3	20 3·2	11·2	20·3
23 1·9	9·9	18·9	13 3·0	10·9	18·9	4 3·9
9·6	17·5	6 2·6	10·6	18·5	28 2·5	11·6
17·2	31 1·2	10·2	18·2	21 2·2	10·2	19·2
24 0·9	8·9	17·9	14 1·9	9·8	17·8	5 2·9
8·5	16·5	7 1·6	9·5	17·5	29 1·5	10·5
16·2		9·2	17·2	22 1·1	9·2	18·1
23·8		16·9	15 0·8	8·8	16·8	6 1·8
25 7·5	April.	8 0·5	8·5	16·5	30 0·5	9·4
15·1	1 0·2	8·2	16·1	23 0·1	8·2	17·1
22·7	7·8	15·8	23·8	7·8	15·8	7 0·7
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DEIMOS.

March.	March.	April.	April.	April.	April.	May.
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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 132. JANUARY. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, December 9, 1887.

J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel and
Lieut.-Col. G. L. Tupman.

The Minutes of the previous Meeting were read and confirmed.

Col. Tupman. 56 presents have been received since the last Meeting of the Society, of which I need only call attention to three. The most important is the continuation of M. Souillart's Investigations on the Movements of the Satellites of Jupiter, just received. The first part was published in our 'Memoirs,' Vol. 45. There is a handsome photograph which has been presented to us by Admiral Mouchez, and represents the distinguished members who attended the Conference at Paris, and our delegates are among the number. There is also a beautiful photograph of a portion of the solar spectrum obtained by Mr. G. Higgs of Liverpool with a 4-prism spectroscope.

A vote of thanks was passed to the donors, and especially to those donors of the presents named by Col. Tupman.

The President. The next business before us is for the Fellows of the Society to appoint Auditors.

Mr. Buckney. I beg to propose that Mr. Inwards, Mr. Sadler, and Mr. Bryant be appointed Auditors for this year. I understand they have been good enough to signify their willingness to undertake the task.

Mr. Hurst. I beg to second it.

The resolution was agreed to.

The President. At our last meeting we suffered from a plethora of papers, and were unable to do justice to four or five of them. We have got the exact opposite of that at this meeting. We have only two papers, and neither of them is suitable for reading. One is a mere ephemeris and one is a mere note on a double star Mr. Christie, the Astronomer Royal, has some remarks to make to us on curved plates; but he is not here yet, so now is the opportunity for any Fellow who wishes to do so to bring any matter before the Society—an opportunity which very rarely occurs.

Col. Tupman. As this seems to be an opportune moment, I wish to relieve my mind of a trifling matter. I thought if I described shortly the kind of micrometer I use for the differential work of an equatoreal it might be interesting. I do not know that anybody else uses precisely the same thing. It is very simple. In the first place, I have mounted on the frame of the micrometer two pieces of watch-spring at right angles to one another, and they are of such a breadth that they extend to about a second, or a second and a half, of time for an equatoreal star. They are placed at right angles to one another. They are broken off a coil of watch-spring. Perhaps I may break off twelve of these pieces. I take them up one by one and try their edges one against another until ultimately I get two which I think to be perfectly straight. Having made them nice and flat I fasten them down by means of screws, which nip each one on to the plate and so keep it in position, and this micrometer is fixed on a good large position-circle 7 or 8 inches in diameter. The method of using it is to determine the reading of the position-circle when a bright star or one near the equator runs along the edges, I find that that is a thing which can be done in practice with very great accuracy indeed. You have a star-disk in the telescope rather bright in the middle and a little foggy round the outside perhaps, unless there is a very clear diffraction ring, and you can carry the bisection of such a star along the edge of such a bar with very great accuracy. After a little practice I found no difficulty in getting these edges within a minute of arc. The individual observations did not differ perhaps more than three minutes of arc from the mean, so that I was quite sure, after allowing a star to run along the edge three times, that I had not any error worth speaking of in the reading of the position-circle when that edge was placed parallel to the equator. One does that kind of thing when one has a spare moment, and I suppose I have done it perhaps twenty or thirty nights and obtained the angle of the bars with very considerable accuracy. I do not suppose there is an error of anything like a minute of arc in the determination of these two angles. Besides that of course by means of the position-circle I can set any one of these edges to any angle I please to the hour-circle passing through the centre of the reticule. In course of observing I find that I get readings in different parts of the heavens due to the flexure of the declination-axis and cradle. That is a point which Bessel has investigated and given a formula for correcting; but I think it is better to allow the comparison star to run along the edge of the bar two or three times and satisfy yourself as to the actual zero reading of the micrometer. You then add 45 degrees to the reading. Each of these bars is then set approximately, making an angle of 45 degrees with the hour-circle. It is not done accurately. There is no necessity to set these bars with very great accuracy to that position, because the readings of zero which you obtain at other times at your leisure and the setting of your micrometer at the time you use it will always determine these angles within about a minute of arc, or say two at the outside. That being so, any error that arises in the observations in consequence of these bars being not exactly correctly oriented can be absolutely eliminated by applying a very small correction dependent upon this angle. The mode of using it is very obvious. You allow one object to transit the bars at one part of the field and another to transit the bars at another. At the moment the object comes to the edges you tap your key and so obtain four marks on your chronograph at the instant of the transit there, and also for your other object you get other marks. The complete observation of both objects generally takes two or three minutes. Then you push your equatoreal forward in the usual way, and repeat it as often as you please, or you shift the two down to the other side, if you see any advantage in doing that. Sometimes, of course, if the difference of declination between the two objects is considerable you must take the second object on the other side of the centre. That is always a slight disadvantage, but it cannot be helped sometimes. Generally, in comparing a comet or minor planet with stars I find it is quite sufficient to take six of these transits. There does not appear to be much gained in taking many. I have done such a thing as take five-and-twenty, although if the comet is rather faint and diffused you are likely to take a larger number. That kind of observation presents a most delightful simplicity of reductions. You see at once you have only to get the mean of these four times and subtract one from another and that gives you the difference of Right Ascension. You take the mean of the first two from the mean of the second two and divide it by two and you obtain the distance of that object from the centre expressed in time. These angles being 45 degrees you have of course the difference of the declination of the object in your reticule expressed in time. The same with the other object. That is very simple. That also is as simple as simple can be, because the angle is always very very small.

(Col. Tupman here explained by means of the blackboard the method of using the instrument.)

The correction for defective orientation is obtained by multiplying your difference of declination as you have got it expressed in time by the sine of twice the error of orientation. This is always, of course, an extremely small angle; but the mere fact of its being the sine of twice the error shows you must be rather particular in orienting such a reticule, because a small error in orientation produces double the effect on the difference of right ascension. There is also an error in measuring the declination, because your angles are not, strictly speaking, right angles. If the northern angle should be a little bit more than a right angle from bar to bar, it is obvious the object will take a little too long to go across relatively. The difference of declination will appear to be a little bit too much. As I have determined the angles with very considerable accuracy, I know that in my particular micrometer this angle is twelve minutes larger than the other. The north angle is twelve minutes longer than the east or west. The correction for declination is equally simple. It simply depends upon the sine of half that angle. There remains the correction for refraction, and that is a thing you do not generally apply unless the difference of declination is very considerable; but I must say the correction of the refraction for this micrometer is very much more simple than that in the case of the ring-micrometer. With the ring-micrometer the correction for each object for refraction depends on four angles, which may be anything, making it a troublesome calculation; but when you use the bars that I speak of it reduces the correction for refraction to its most simple form, because each of the angles at the points where you have to apply refraction is 45 degrees.

There is another point also about it which appears to me to make it superior to the ring-micrometer. I compare it with the ring-micrometer because they are similar to each other in principle and one may really be regarded as a modification of the other. The rings are so placed as to suffer the maximum amount of indistinctness from the imperfections of the eyepiece. You must use an accurately constructed achromatic eyepiece in order to see the edges of the ring sharply defined, and I do not think anybody ever does see the edges of his rings defined with the degree of sharpness that they ought to be. When you use bars they are radial to the circles of distortion, and the consequence is you have the best definition possible of the edges of the bars, and in addition to that you use the centre of your field as much as possible, whereas with the ring your observations are generally made—necessarily made—as far away as possible from the centre of the field, so it is what may be called a modification of the ring, a very advantageous modification; but of course it obliges the use of the position-circle. Everything depends on the degree of accuracy with which you connect the position of the bars with the reading of your position-circle. I have used such a micrometer now a great many years, and I am able to say positively it gives good results; if you compare two stars you will get results quite as accurate as are given by any other micrometer.

The President. A discussion on the different kinds of micrometer is perhaps one of the most interesting discussions that could be brought before this Society, and I will invite any Fellow to make any remarks upon Col. Tupman's communication.

The Astronomer Royal. I quite agree with Col. Tupman as to the value of this micrometer. It seems to me that it is much more accurate and more useful than the ring-micrometer, and we have adopted it at Greenwich for some years past for comet observations with satisfactory results. To facilitate the orientation of the cross-bars, I have had a micrometer made with a finer wire bisecting the angle between the bars, and the micrometer was set on every occasion by allowing a star to transit along the wire, so as to get rid of any errors of orientation in these bars.

Col. Tupman. You depend upon the maker making those angles 45 degrees.

The Astronomer Royal. Yes, and I think it can be done with sufficient accuracy. In the observations we made we found we could depend upon the equality of these angles to a sufficient degree of exactness. It is obvious that that can be tested in various ways, either by means of the position-circle which one can depend upon, or by means of known stars. At Greenwich this micrometer has been used for comet observations, where the degree of accuracy required is not so great as in those observations Col. Tupman has referred to.

Mr. Turner. There is one point in the observations of comets in connection with this eyepiece, which has sometimes seemed to me to interfere with the accuracy of the observation. We have several times had brought before us by Dr. Gill and others the difficulty of bisecting an object with a wire, and Dr. Gill has introduced a reversing prism eyepiece for the purpose of eliminating any error of bisection which may occur by reason of the particular orientation of the wire. In this case we have two wires of contrary inclination, and it seems to me that an observer very likely has a tendency to consider the comet bisected when it is beyond one wire, and again when it has not yet reached the other, in which case you get the whole effect of declination doubled. I cannot say that I have any conclusive evidence on that point; but in investigating the results of comet observations at Greenwich, and from observations I have made myself, it seems to me that has possibly occurred in several instances.

Col. Tupman. To a certain extent the error Mr. Turner alluded to does not exist when your bars are broad like mine. Small comets wholly disappear behind the bar, and there is a time during transit when you see the whole of the comet and you have to guess afterwards that the whole comet has appeared on the other side. It is the mean of these two observations which will be the transit of the comet over the bar. When the comet is very large and diffused, it is extremely difficult to observe it in this way, and probably then one good thick wire with an estimate of the instant when it is over the centre of the comet would be the best way; but when the comets have a small stellar nucleus, 2, 3, or 4 seconds in diameter, you can observe them very nicely indeed by that method.

Mr. Turner. I quite agree with Col. Tupman about the well-defined nucleus, but with a faint object one is so liable to lose the comet until it is some distance past the wire, and that is a kind of thing which makes this big error of bisection in the observation of the wires.

Mr. Ranyard. Mr. Carrington in his observations of the Sun used wires in the observation of the spots, and he preferred thick wire, the sun-spots being very irregular.

Col. Tupman. Something like what Neison proposed for a micrometer for Moon work, but he did not use the position-circle. What I wished to call attention to was the orienting of the bars accurately by means of the position-circle.

The Astronomer Royal. There is another modification I have tried, and that is to use two bars with an interval between, and that gives more accurate observations of a star. You can observe its disappearance and reappearance, or the simple reappearance between the two, which is an almost instantaneous phenomenon, and you can also observe a diffused comet. I mention that as a suggestion. I am not clear that there is any very great advantage from it, but there may be under certain circumstances.

Mr. Brett. Rather difficult to make?

The Astronomer Royal. Not particularly. I have had a micrometer made with one double horizontal bar and five vertical ones, and have adjusted the bars myself without difficulty.

Capt. Noble. That observation of the reappearance in the field between the bars is a very puzzling one. I think it was Mr. Thornthwaite who invented a flashing eyepiece. I never got within ¾ of a second of time with it.

The President. I am sure the Society will be very glad that Col. Tupman has availed himself of this opportunity of relieving his mind. I think the discussion has been an interesting one, and I am sure the Society will give its thanks to Col. Tupman. (Applause.)

The Astronomer Royal. I hardly like to trouble the Meeting again with the question of curved plates, but at the last Meeting we had not an opportunity of fully explaining what had been done at Greenwich, and since then Mr. Criswick has succeeded in obtaining some more photographs of stars on curved plates, and I thought it might be interesting to the Fellows to hear how we are progressing with this work. I may recall that the instrument with which these photographs are taken is Dallmeyer's 4-inch object-glass of 60 inches focal length, corrected for the photographic rays, and that the plates are curved to the curvature of the field, taking the locus of the circle of least confusion. The radius of curvature of the field represented by the locus of the circle of least confusion is about three-eighths the focal length, that is, about 22 inches. Now the plates that have been taken are 6-inch by 6-inch, covering about 5¾ degrees square. Mr. Criswick has taken several successful photographs lately of the Pleiades group in the following manner:—An image of the group is taken at the centre of the field with an exposure of ten minutes. The instrument is then displaced in the direction of a diagonal through a definite space (105″), and another image is taken with an exposure of two minutes. The group is then photographed in one corner of the plate with two exposures, a displacement of the same amount and in the same direction being given as before, and then again in the opposite corner with two exposures as before.

The object of that was to give means of determining the distortion on the different parts of the plate, and especially to see whether the distance between two stars would change appreciably with the length of exposure; that is to say, whether as the image of the star grows, the centre of condensation would always remain at the same point, and the distance between the two images would be the same for bright stars as it would be for faint ones. I ought, however, to say we have not yet made any attempt at measurement of these plates. There is a great deal of difficulty in getting an instrument that would measure them properly, and I am not prepared to say now that we shall attempt it. The real object in taking these photographs was to compare the curved plates with the flat plates, and to see whether we could get a greater extent of field. I find that in these curved plates one can get quite measurable images—I do not say circular disks—about three degrees from the centre of the field, so that we can get a field about six degrees in diameter with measurable disks. It is a feature of interest in the matter that at a distance from the centre of the field the circle of least confusion is not by any means a circle, but more like a cross, at any rate with this object-glass. I am not going to generalize, because I think a good deal depends upon the object-glass. Also, as one goes further from the centre, there is an extension in one direction. The image is winged, something like a kite in fact. As the exposure is increased this fills up a little, and you get something of an approach to an oval; but the geometrical condition of the circle of least confusion on the plate seems to me to be merely that these two lines at right angles are of equal length, and, as a matter of fact, it is not a circle at all, but a cross representing the two diameters of a circle at right angles. We propose after this to get some more photographs on flat plates for discussing the question of how the point of greatest density of the image changes as the exposure is prolonged, to see if there is any sensible displacement.

Capt. Noble. May I ask the Astronomer Royal what the curvature of the plate is? Is it part of a cylinder?

The Astronomer Royal. It is part of a sphere as near as one can get it.

Capt. Abney. I think there may be something in this extraordinary form that the star assumes relative to the plate, due to the fact that the thickness of the film containing the sensitive salt is not taken into account. I have had a great many experiments with regard to this question, and it has always been assumed that the thickness of the film is measurable. In the old collodion days the film could be measured in ten-thousandths of an inch.

In the present day you may see sensitive plates on which the film instead of being measured by ten-thousandths must be measured by hundredths, and there is a very great deal of irregularity in the thickness of the gelatine. This is not a theoretical, but a practical result, which you can try if you remove the gelatine film from the plate yourself; you find you get what is called halation very strongly marked in these films, although you will not get it in the collodion films when you remove them.

I think the Astronomer Royal will find that the refraction and the reflection due to the film will be very apt to give the kind of figure of which he has spoken. It is a very well-known figure when you are taking images on the extreme margin of the plate; but if you take a star-disk such as you have there, and give a much longer exposure, you will eventually get not a circle, but an elliptical or oval form.

Mr. Common. With the axis which way?

Capt. Abney. The longest part of the oval will be away from the centre. In the same way if you have such a thing as a triangular disk or star, you will find you get the same thing exactly, you will get an oval form of star. This is a matter of simple experiment.

The Astronomer Royal. With what object-glass or lens? not a single lens?

Capt. Abney. No.

The Astronomer Royal. What focal length?

Capt. Abney. Six feet. I cannot speak of anything beyond that. I can only say this does happen, that whatever aperture you have, if you have a square or a very small triangle, you will still get an oval disk. That is simply a matter of experiment. I have gone through these experiments for another purpose, and that was the result; and if you remove the film from a plate, you have the same result, though not so markedly perhaps as when the film was on the plate, which simply shows that as regards the sensitive plates which are prepared now you cannot neglect the thickness of the film. In the old collodion days you could neglect it, because it was so very thin, but the gelatine film is very much thicker than the collodion.

Mr. Turner. May I ask if there is distinct evidence that these forms are due to the thickness of the film?

Capt. Abney. I do not say that at all. The Astronomer Royal said that these parts were filled up gradually.

Mr. Turner. Do you mean the filling up of the original cross form?

Capt. Abney. The filling up. You will find it makes a difference, that there will be a difference, as the object will move away from the centre of the field as the exposure is prolonged. That is the point I am trying to drive at. The image will move from the centre of the field, so that you will not get the same distance between two stars.

The Astronomer Royal. Of what distance from the centre of the field are you speaking?

Capt. Abney. I will not give it in angles, because I really cannot tell what it is in angles. With a 5-inch square plate you find 4 inches in the field; 2 inches from the centre you will get a movement of a bright disk away from the centre. So far as my experience goes it must be so in the stars.

The Astronomer Royal. Perhaps I might supplement what I said before. I quite agree with Capt. Abney that in all this discussion one has to bear in mind two sources of error—one arising from this reflection from the back of the plate, from the chemical phenomena of halation, and the other from optical astigmatism. The effects which I have been calling attention to here seem to result from optical astigmatism, because when we shifted our plates, trying different positions for focus, we got different forms. [The Astronomer Royal here showed on the blackboard the shapes the star-images had at the margin of the plate, and how they changed in form.] Now these figures, it seems to me, follow directly from the ordinary laws of optics, whether refraction or reflection, and it is not at all necessary to take into account the reflection from the back of the film or the thickness of the film. I do not know what thickness of film there might be, but the difference in the focus between one focus and the other would be one-tenth of an inch. If the thickness of the film is only a few thousandths of an inch or something less than the one-hundredth of an inch, it strikes me that is hardly sufficient to explain such large effects or rather differences of effects.

Capt. Abney. I am not disputing the optical effects; but the question raised, as far as I can make out, is whether the apparent centre of the figure moves on a bright star; that is to say, if you have a faint star and a bright star close together, whether the distance apart would be the same at the centre of the field as at the edge of the plate.

The Astronomer Royal. Yes.

Capt. Abney. I say that it will not.

The Astronomer Royal. I think it very likely it will not, but I do not think I quite intended to raise it in that form, and to say that they would be the same. Our object was to investigate the correction that would be required, and to do that by actual experiments. However, I quite admit the value of Capt. Abney's suggestion about this point, and this is a matter to be borne in mind. The point I wished really to draw attention to is this: that the so-called circle of least confusion instead of being a circle is a cross in this particular case, and I fancy in many other cases.

Mr. Knobel. I should like to ask Capt. Abney whether in his remarks on the thickness of the film be referred to hand-coated or machine-coated plates?

Capt. Abney. Exactly the same. Probably worse in the machine-coated, because there is more gelatine in proportion to silver.

Mr. Ranyard. I made some experiments some time ago which have a bearing on this point. I coated some plates with collodion with a very thin coat, and I was not able to find any difference in the irradiation from bright images, even when very much extended, and with a short-focus instrument which gave very broad fluffy edges, and some other experiments of mine seem to show that the Astronomer Royal's suggestion as to this being a purely optical condition of the image is probably true, but photographs taken with the reflector when sharp showed the six rays most beautifully. In those shown by Mr. Isaac Roberts there were eight rays most distinctly shown, just as you see them with the eye; and also I have evidence that a star-disk may be cut across by a piece of platinum foil being placed upon the front of the plate, showing that if any action took place within the film, and depending on the thickness of the film, we should have all this eating this way under the opaque screen in front, but it does not; it is cut off sharply; therefore I think it is simply due to the optical part of the image in front, and that there is no very appreciable extension within the thickness of the film.

Mr. Common. With reference to what the Astronomer Royal has said in connection with an object-glass of the ordinary single combination of a crown and a flint, I think that no one who has made any experiments expects a very large field from that form, even by correcting the great curvature of the field by curved plates. Some years ago I made a series of experiments that I gave in a paper here, trying many kinds of double combinations that gave a flat field, in fact in some cases even a field curved the other way, and in these the shape of the image at the margin of the field was fairly oval, but with the major axis at right angles to the line from the centre of the plate. With regard to the image like a cross filling up, as the Astronomer Royal says, with longer exposure, the tendency is always to fill up such a shape as a cross, so that whatever the shape of a bright point that is photographed, the tendency of photography is to make of it more and more like a circle.

Capt. Abney. I am glad Mr. Common has spoken, because he has just confirmed what I said, although he does not think he has. He says by copying the plates successively if you have got a triangular star it will eventually become a circular disk. That is due to the reflection, not necessarily from the back of the plate, but from the back of the film. [Captain Abney explained by means of the blackboard the effect of reflection from the film and the back of the plate in changing the form of images.] But I must ask you to think that besides this glass plate you have got another transparent medium which is very thin. If you will take tinfoil or platinum foil, and cut it out very carefully, and place it in close contact with the plate, you will find if you take a triangle you will get an effect of halation, as you will if you have a cross. It has been measured, and it comes out as it should do. It is the thickness of the glass. But at the same time you also find there is a brightening of the image at the edges of these plates, and that is due to the halation from the thickness of the film, and you may take that film off the glass and expose it in the same way, and you will still get that thickening round the edge of the image.

Col. Tupman. You get it on the paper?

Capt. Abney. Yes: that is what Mr. Ranyard said, when he put the platinum foil across the star he got no encroachment. I have not the slightest doubt his observations were correct, but I would ask him if he saw a brightening of the image at the edge.

Mr. Ranyard. I compared it with a slit, which is more critical than the star. With different thicknesses of collodion film I was not able to see it.

Capt. Abney. I am not talking about collodion film, I am talking about gelatine. I said the old collodion film did not give you this difficulty when you measured by the 10,000th of an inch instead of by the 100th. I have taken bright images of known area, and measured them with the micrometer, and put them on films, and I have photographed them on films without any backing whatever, and as a matter of fact the light encroaches upon the space very considerably. I am not prepared to say that by degrees copying and re-copying you will not eventually fill up the spaces altogether in the same way as the Astronomer Royal said. This is due to the reflection from the back of the film and from the back of the plate; that is putting aside the optical phenomena altogether, and I think in a matter of this kind astronomers cannot be too careful as to knowing exactly what they are doing. This halation is a matter worthy of study, because it will affect not only the beauty of the star photograph, it will also affect the star magnitude very considerably.

Mr. Common. True.

Capt. Abney. I am glad to hear Mr. Common say true; I have made a very great many laboratory experiments which you can very well perform on artificial stars. Those same kind of holes Dr. Spitta had to bore, I have had to bore, and with the results I have told you. When you get to the edge of the field the encroachment is greater. You have greater exaggeration of the star-disk in this direction [blackboard illustration] than you have in the other. A matter of vital importance in making star maps is this matter of halation, and those who undertake them should make this a preliminary study.

Mr. Common. Last year, when using a lamp and small pinhole for testing mirrors, I placed a 4-inch object-glass at twice its focal distance in front of the illuminated pinhole, and I got the image from this very point. I got an image I could deal with; it was perfectly steady. I made some tentative experiments as to the photographic action on the plates. I perforated a piece of platinum foil with a groove $\frac{1}{50}$th of an inch. A momentary exposure of the point gave an image comparable to the image of the pinhole, as it was a very good lens; and with continued exposure of two to ten minutes I got larger and larger images, until I got a magnitude that would very much encroach upon the groove I had cut in a piece of platinum foil, and I then carefully arranged the platinum foil so that the image would fall in this groove. Then I exposed the plate for ten minutes, but this photographic action, which we might consider to be due to reflection from behind, I could not get when I had the opaque screen in front.

Mr. Ranyard. Mr. Common's remarks entirely corroborate what I was saying, that the diffusion of the image due to the badness of the optical image is very much larger than the diffusion in the film. I cannot say I found any definite evidence of diffusion within the film even without this thick film round it. The chief effect is due to the badness of the image upon the film rather than to the diffusion of light within the film.

Mr. Common. I would not attach any great importance to these experiments, because they were only made tentatively.

Mr. Inwards. It seems to me it must be very difficult with these concave plates to get a film of even thickness. There would be a natural tendency for the film to be thicker in the deepest part of the plate.

The Astronomer Royal. I may say that a difficulty was experienced at first in putting on the film, and the first batch of plates we had were not so sensitive as the ordinary flat plates; but we have since had another dozen prepared, and they seem satisfactory, both as regards the evenness of the film and the sensitiveness. They are nearly as sensitive as ordinary plates. I should suggest if these plates were to be adopted—not that I should be prepared to advocate them, for it is a question of further investigation—that they should be rephotographed by means of the same object-glass with which they were taken. I may say a word on the question of distortion, to which Capt. Abney has alluded. There is one point with regard to the ordinary single combination, and that is, that the point of greatest condensation is the point at which the axis of the pencil falls on the plate. If you can see that in the image and identify it, that point will be free from distortion. The point that our experiments were directed to was to try and find, in this peculiar image I have described, what was the point of greatest condensation in the image. I am inclined to think one could identify that point pretty clearly, and I am not certain, even after Capt. Abney's explanation, that that point would change with the progressive increase of exposure. The question really is this, not to find the centre of this irregular image, but to find the point where the rays are most condensed. That is another question, which may or may not be affected by this halation.

I think it is a point we want to discuss very carefully before entering upon this question of star-photography, with the intention of forming a map.

The President. I am sure we have had an extremely interesting discussion, and I am very glad Mr. Christie has brought the matter before us. I think also it was a very fortunate circumstance indeed that Capt. Abney was present.

Mr. Knobel. Seeing the paucity of papers this evening, I think it might perhaps be interesting to the Fellows to be informed of the researches on Ptolemy's Catalogue of Stars recently undertaken by Dr. Peters of Hamilton College, Clinton. Dr. Peters has visited the principal libraries on the continent—including the Vatican library, the Laurentian library at Florence, the libraries of Vienna, Venice, Paris, Berlin, and other cities—for the purpose of collating all available existing MSS. of Ptolemy's catalogue. The best edition of Ptolemy's catalogue we possess is that published in the 13th volume of our Memoirs by Mr. Baily. That edition is simply a collation of the *printed* editions of Ptolemy—these are the Liechtenstein edition of the Almagest published in 1515, which is in Latin and is a copy of the translation from the Arabic by Gerard of Cremona, made in the 12th century. The next edition is that of Trapezuntius, published in 1528, also in Latin, but translated from a Greek MS. at the Vatican. The third edition employed by Mr. Baily was the well-known Greek Almagest published in 1538, and which is a transcript by Grynæus of a Greek MS. which belonged to the well-known astronomer Regiomontanus at Nuremberg. The last work which completed the data employed by Mr. Baily was the edition of Halma in Greek, published at the early part of this century, and which is mainly derived from a remarkable 9th or 10th century Greek MS. at Paris. But Dr. Peters has far enlarged the materials for constructing an edition of Ptolemy's catalogue; he has examined and collated some 16 MSS. in Italy—at Rome, Florence, and Venice—also MSS. at Vienna, besides others at Paris and elsewhere. I have undertaken to cooperate with him* in regard to this country, and have succeeded in collating most of the available MSS. of the Almagest that we possess in England. These comprise 2 Arabic Almagests, at the British Museum and at Oxford—a Greek MS. of the 14th century at the Bodleian Library —3 Latin MSS. of Gerard of Cremona's translation from the Arabic of the 13th, 14th, and 15th centuries—and 5 Arabic MSS. of Al Sûfi, in which the positions of the stars differ from those in the Almagest only by a fixed constant. These materials I am sending on to Dr. Peters, and he will then have the collations of nearly all known MSS. of the Almagest for constructing a far more accurate edition of Ptolemy's catalogue than we possess at the present time. It is especially satisfactory that Dr. Peters has undertaken the work; he has gone most elaborately into the subject and has rigorously computed the places of the stars back from modern catalogues and compared the positions with those in Ptolemy, and the results he has foreshadowed cannot but be of the greatest interest. I thought it might be of some interest to the Fellows to know of the progress of this work.

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* Six years ago I had quite independently commenced the collection of materials for constructing an edition of Ptolemy's Catalogue derived from oriental sources, believing, for several reasons, that the most reliable data would be found in Arabic MSS.—E. B. K.

The President. The Society has already thanked Mr. Knobel for his extremely interesting account of this most important work. I am sure we shall all be very glad he has been able to give valuable assistance to Dr. Peters, and also for giving us so interesting an account of his work.

The following papers were announced:—

W. H. S. Monck. "Notes on the Orbits of the Asteroids."
J. E. Gore. "On the Double-Star Σ 1847."
A. Marth. "Ephemeris for Physical Observations of Mars."

J. W. F. Allnutt, B.A., Gainsford Bruce, Q.C., Major Somerset H. Maxwell, and the Rev. S. H. Parkes, B.A., were duly elected Fellows of the Society.

The Meeting adjourned at 10 o'clock.

Meeting of the Liverpool Astronomical Society.

The third meeting of the Session was held at the Royal Institution on the 12th inst., Mr. James Gill in the chair. Twenty-one members were proposed and elected.

Mr. W. F. Denning, F.R.A.S., read a paper on "Comet-seeking.' He remarked that there was probably no section of practical astronomy more attractive, or which held out a greater prospect to amateurs than this; besides there were Mr. Warner's prizes to stimulate, interest, and impart a substantial benefit to successful observers. Yet it must be acknowledged that there were few who applied themselves seriously to this work. Probably there was not a single observer in Europe at the present time who was systematically and thoroughly engaged in sweeping for comets; or if there was, there must be something radically wrong with his method, judging from the paucity of results. All the spoil seemed left to two able American observers—Barnard at Nashville, and Brooks at Phelps, New York, who seemingly vied with each other in characteristic zeal and pertinacity. In six years they had discovered eighteen comets—nine a-piece—and had received over 2700 dols. in prizes. Many comets undoubtedly eluded us altogether, owing to the paucity of observers; still it was a satisfaction to note that during the last six decades the rate of cometary discovery had been much augmented by the assiduity of a few enthusiasts. During the decade 1827 to 1836 only 14 comets were discovered, as against 49 in the decade 1877 to 1886. Any telescope between 4-in. and 10-in. aperture could be used as a comet-seeker, though reflectors were preferable to refractors; unless a prism, as used by Mr. Barnard, was employed with the latter form so as to give a side view. The observer must possess a copy of Sir John Herschel's "General Catalogue of Nebulæ and Clusters of Stars," as, during the process of sweeping, many nebulæ would be encountered. Comets may appear on any part of the firmament; but their paths converge towards the Sun's place, so that the most promising regions for search were at low altitudes in the west after sunset, and in the east before sunrise. Whenever a comet is found, the observer should at once give notice by telegraph to the Astronomer Royal at Greenwich; and as soon as possible, by letter, to Prof. Swift, of the Warner Observatory, Rochester, New York, and thus render himself eligible for the 100 dol. prize.

Mr. James Gill remarked that the great difficulty with amateurs was to distinguish a comet at first sight from a nebula. Mistakes of this kind had often been made, and they were very discouraging. Mr. R. C. Johnson, F.R.A.S., thought that there might be some particular region which comets were more likely to cross. It would be interesting to note the positions of comets when they were first discovered, as this might be of assistance to amateurs. After some further discussion, Mr. Johnson promised to read a paper on the subject at the next meeting.

In the course of a paper on the "Guericke and Parry Group of Walled Plains," Mr. T. G. Elger, F.R.A.S., said that the best time for observation was when Bonpland, on the east of Parry, was partially obscured. The illumination of Guericke was then most favourable, as the bright, narrow, broken walls nowhere exceeded 2000 feet in altitude, and the objects on its floor were only seen with difficulty under a more vertical sun. There was a want of continuity of the wall on the north, where a gap almost as wide as the ring itself is occupied by lofty isolated masses of rock.

Papers were also read by Mr. Lynn, B.A., F.R.A.S., on the "Rotation of Jupiter"; by Major Skelton on "An Improved Altazimuth Mounting"; by Mr. A. Stanley Williams, F.R.A.S., on "Light Curves and Streaks on the Floor of Plato"; and by Mr. W. F. Denning, F.R.A.S., on "Saturn."

It was decided to hold an educational meeting each month, after the ordinary meeting, under the direction of Mr. James Gill. Mr. J. Quilliam was appointed secretary to the branch.

Royal Meteorological Society.

THE usual Monthly Meeting of the Society was held on Wednesday evening, the 21st inst., at the Institution of Civil Engineers, 25 Great George Street; Mr. W. Ellis, F.R.A.S., President, in the chair.

The Rev. R. Barker, Mr. W. W. Day, M.D., Mr. H. N. Dickson, Mr. H. Harries, Mr. P. S. Jeffrey, B.A., Mr. H. A. Johnstone, and Mr. J. Wolstenholm were elected Fellows of the Society.

The following papers were read:—

I. "The Mean Temperature of the Air at Greenwich, from September 1811 to June 1856," by W. H. S. Eaton, M.A. This is a discussion of the Meteorological Journals of the late Mr. J. H. Belville and those of the Royal Observatory. The general results of the investigation are:—1. That there was no appreciable change in the mean annual temperature of the air at Greenwich in the period 1812 to 1855 inclusive. 2. That on the eminence on which the Royal Observatory is situated the average temperature at night, or rather in the early morning, is in all cases higher than over the lower grounds. 3. That with a north-wall, or possibly a north-window exposure, higher maximum temperatures are found at the lower stations. 4. That the movements of the thermometer are retarded with a north-wall exposure as compared with an instrument on an open stand, especially where the situation is a confined one, the indications of the thermometer not following changes of temperature so promptly, owing to the modifying influence of the adjacent building.

II. "Report on the Phenological Observations for the year 1887," by the Rev. A. T. Preston, M.A. The past season was a most exceptional one. For flowers it was disastrous; fruit was generally a failure, though there were exceptions; those kinds which promised well turned out very small or were spoilt by insects. Vegetables were universally poor, roots were destroyed by insects or drought, and green crops soon passed off. The wheat crop, however, was better than was expected. Barley on light lands was poor, but that which was sown early was satisfactory. Meadow hay was not up to an average crop, but clover and seed hay were very nearly so. In Kent the fruit crops turned out lighter than usual, but the prices ruled higher.

III. "Earth Tremors and the Wind," by Prof. John Milne, F.R.S., F.G.S. The author has made a detailed examination of the Tremor records obtained in Tokio, and compared them with the tri-daily Weather Maps issued by the Imperial Government of Japan. From this comparison the following conclusions have been drawn:—1. Earth tremors are more frequent with a low than with a high barometer. 2. With a high barometric gradient tremors are almost always observed, but when the gradient is small tremors are seldom perceptible. 3. The stronger the wind the more likely are tremors to be observed. 4. When there has been a strong wind and no tremors the wind has usually been local, of short duration, or blowing inland from the ocean. 5. When there has been little or no wind in Tokio and tremors have been observed, in most cases there has been a strong wind in other parts of Central Japan. 6. From 75 to 80 per cent. of the tremors observed in Tokio may be accounted for on the supposition that they have been produced either by local or distant winds. 7. The only connection between earth tremors and earthquakes in Central Japan is that they are both more frequent about the same season.

IV. "Pressure and Temperature in Cyclones and Anticyolones," by Prof. H. Hazen. The author has made a comparison of the observations at Burlington and on the summit of Mount Washington, U.S.A., and as the result of a study of about 4000 observations from two days before till two days after the passage of cyclone and anticyclone centres, he has arrived at the following conclusions:—1. In both cyclones and anticyclones the pressure lags from 10 to 11 hours at the summit of Mt. Washington. 2. The temperature change at the base precedes very slightly the pressure change, but at the summit the change occurs nearly 24 hours earlier. 3. The temperature change appears very little earlier at the summit than at the base, and certainly varies much more rapidly at the former. 4. In a cyclone the difference in temperature between base and summit is less than the mean before the storm, but the difference rapidly increases after the centre has passed. Just the contrary is true in an anticyclone. 5. The total fall in pressure in a cyclone at the summit very nearly equals that at the base, and likewise the rise in an anticyclone. 6. The fluctuation of temperature, that is from the highest to the lowest, at the summit is double that at the base in a cyclone, but it is only a little greater in an anticyclone.

The Astronomical Society of France.

The December meeting of the above Society was held on the 14th, M. Flammarion, President, in the Chair.

The meeting was opened with the distribution of the Calendar Reform Prize, consisting of medals and money to the value of 5000 francs (an anonymous gift).

1st	Prize,	1500	francs:	M. Gaston Armelin, Paris.
2nd	”	1200	”	M. Hanin, Auxerre.
3rd	”	1000	”	M. Francis de Roucy, Compiègne.
4th	”	800	”	M. Barnout, Paris.
5th	”	250	”	M. Remy Thouvenin, Nancy.
6th	”	250	”	M. Blot, Clermont (Oise).

M. Flammarion read a paper on "Some probable common Proper Motions of Stars." In looking over the Catalogue of the Paris Observatory, he had observed that several stars in Taurus, viz. Lalande 8178, 8209, 8237, 8256, 8297, 8404, had no proper motion in declination and had all about the same motion in R.A., thus indicating perhaps a sort of star-drift. The same remarks apply to θ^1 and θ^2 Tauri. The two stars γ Leporis and Lalande 10931 seem also to be connected.

Colonel Laussedat, director of the *Conservatoire des arts et métiers*, exhibited a curious binocular glass constructed for Louis XIV. in 1681 by Father Seraphin. This huge instrument measures no less than 3 metres 10 cm., and is composed of three rectangular boxes which slide into each other.

M. Gaudibert sent a fine drawing of the lunar crater Hevelius.

Prof. Dufour, of Lausanne, and M. de Boë, of Antwerp, sent some remarks on the forthcoming eclipse of the Moon of Jan. 28, 1888.

M. Neuville, in a letter, remarked that the minimum of Algol seems longer than 6 minutes, as given by several observers, and is more likely 18 minutes. MM. Paul Henry and Detaille remarked that 18 minutes is the number given in Webb's 'Celestial Objects.' M. Neuville's letter gives an estimation of the size of Algol's dark companion.

M. Lutz exhibited a new form of equatoreal stand for small instruments.

Meteor Notes.

November proved a very cloudy month and not many shooting-stars could be observed. The night of the 23rd, however, proved clear, and I recorded 45 meteors between 13^h 20^m and 17^h 50^m. Several paths were traced to the following radiants, some of which also appear to have supplied a few meteors on the nights of November 14 and 15, and in previous years I have registered others from the same showers.

No.	Date, 1887.	Radiant.		Notes and Comparisons.
		°	°	
1...	Nov. 14–23	65	+23	Slowish. Taurids.
2...	Nov. 14–23	132	+31	Swift; streaks. D. 133° +31°, Nov. 10–12, 1877.
3...	Nov. 14–23	141	+27	Swift; streaks. S. & Z., 142° +30, Nov. 19.
4...	Nov. 23	181	+25	Swift; streaks. Coma Berenices.
5...	Nov. 14–23	194	+67	Swift; streaks. D., 194° +67°, Dec. 18–28, 1886.
6...	Nov. 23	200	+56	Slow. Denza, 207° +53°, Nov. 11–15.

Feeble showers were also seen at 77° +33°, 157° +49°, and 169° +74°. Mr. D. Booth at Leeds observed 24 meteors on Dec. 10 between 15^h 50^m and 19^h, and these included 10 Geminids from a radiant at 113° +34½°. A few other meteors indicated a secondary radiant at 111° +21½°, and there seemed a third far north in Camelopardalus at 110° +78°.

Large Fireball.—A meteor of exceptional brilliancy and magnitude was observed in the twilight at about 5^h 10^m P.M. (Dublin time) on November 17. It was widely observed in Ireland, and appears to have been first seen when over the Irish Sea, passing from thence vertically over Drogheda, and moving due west, over Connaught, finally disappeared above the Atlantic. A considerable number of descriptions of this fireball were published in the 'Irish Times'; these, together with some other notices, were obligingly communicated to me by Mr. W. H. Milligan, of Dublin. It may be interesting to quote from a few of these accounts:—

Cleggan, Co. Galway, R. W. Smith. "I saw the meteor as nearly as possible at 5^h 10^m P.M., at apparently not more than 30° alt., between me and 'Letter' mountain, and at no great elevation over same. The meteor exactly resembled a rocket at first, but in a second or two it grew a much more intensely bright light, whitish blue and a nucleus was visible. It was in my sight for say 40 seconds, and once during this interval it lost its light completely, and then shone out again, and I finally lost sight of it distinctly *between me and a bank of clouds*, cumuli, extending a short distance above the horizon. It seemed to be travelling approximately parallel with the earth's surface. The alt. of the meteor when I last saw it would be not more than 12° or 15°." [From a chart sent with Mr. Smith's description it appears the meteor first appeared in azimuth E. 10° N., and disappeared in N. 47° W.]

Duleek, Meath, Major H. C. Gernon. "I saw the meteor of Thursday, Nov. 17, while riding home about a mile W. of Duleck, at 5.15 P.M. It was quite dusk and the sky cloudless; suddenly the heavens became illuminated, and on looking up I observed a ball of fire of a bluish colour (like an ascending rocket) with a long tail—say 6 yards long—coming over my head from due east. It travelled slowly across the heavens, apparently very low down towards the west-nor'-west, and finally disappeared in some fleecy clouds on the horizon. I could see it apparently descending through the clouds, and am certain it must have been in my clear view for 30 seconds. I never saw anything like it. It had a tail exactly resembling a pennant floating from the mast of a man-of-war."

Mountrath, Queen's County, J. Wilfrid McClure. "At about 5 P.M. I observed the meteor come out immediately above the evening star [Capella] and shoot with great rapidity in a W. direction, with a little N. in it, say W.N.W. As it passed through the sky it left a long trail of light after it, yellow near the body of the meteor, blue farther away from it, and finally ending in sparks of red and blue. I watched it closely till it sank behind the clouds; it passed through a dark cloud without being in any way hidden by it, so it would seem as if it were nearer the earth than the cloud. It continued as vivid as at first, till it disappeared behind clouds close to the horizon."

Bray, Co. Wicklow, W. R. Moorhead, M.D. "The meteor or meteorite of Nov. 17 was seen here by myself and some others.

The time was about 4.50 P.M.; it was travelling, seemingly with no great velocity, due west, and in a line parallel with the earth's surface. When I first observed it I imagined it to be a rocket; it was visible here for about one minute."

Sutton, Dublin, J. D'Arcy-Irvine, Commander, R.N. "At about 5 P.M. on Nov. 17 I was walking on the W. side of the Hill of Howth and saw what I at first thought was a rocket coming as though from the top of Howth Hill. The head of the meteor was a large ball of fire, to which was attached a long tail of light pointed like a shark's back, and each joint emitting sparks. It went slowly along in a westerly direction, and almost parallel to the plane of the earth, till it got over Raheny, when it seemed to take a downward direction, and I lost sight of it. I think it was a full minute from the time I first saw it until it disappeared."

West End, Mallow, J. Golding. "The meteor of Nov. 17 was seen here at about 5.10 P.M. It appears to have been a large ball of fire and to have had a long tail. It started at about 20° E. of the meridian, and travelled in a westerly direction about 30° W. It was at its first appearance, as well as I can understand from the descriptions, about 25° or 30° above the horizon, and fell in its course about 10°. It quite illuminated the sky, though it was not at all dark at the time."

St. Ita's, Newtown Park, Dublin, J. H. Archer O'Brien. "At about 5 P.M. on Nov. 17, on returning from a visit near the Scalp, and when a short distance on the Dublin side of the road leading to Carrickmine's station, a large meteor crossed the highroad. It was travelling W. and very slowly, the body and tail, which was long and slightly curved, were of a bright blue and green colour. It gradually paled as it came near the earth, and appeared to fall near Glencullen."

Brittas, near Tallaght. "A Correspondent" writes that: "Last evening [Nov. 17] a meteor of great size and brilliancy moved slowly across the sky from S.E. to N.W. It remained in view from 12 to 15 secs. during its course. The meteor drew after it a tail of considerable length and brightness. The arc of the flight, if the slow movement can be so termed, visible to us subtended an angle of fully 120°. It may be added that the meteor blazed up several times during its passage across the sky, the colour varying from red to blue."

Ashgrove Lodge, Kingstown. "Inquirer" says: "A brilliant meteor was noticed there at about 5 P.M. on Wednesday [Thursday] afternoon. It first appeared towards the N. part of the heavens at an altitude of 30°, and after travelling in a W. direction at great speed over an arc of some 50°, finally disappeared into space at an alt. of about 20°. Its appearance resembled a comet, somewhat of a faint blue tint."

Garville Avenue, Rathgar. A. F. C. Thorp writes that, "on Nov. 17, at exactly 5 P.M., a brilliant meteor was seen there going in a N.W. direction. It had a long tail, and ultimately disappeared in a cloud." At Silverdale, Queenstown, Co. Cork, the fireball was observed by Mr. J. Whiteside, who says "it appeared two or three minutes before 5 P.M., crossing the River Lee and travelling in a S.W. direction." At Drogheda the meteor is described as passing E. and W. directly over the city.

The real path of this fireball seems to have been unusually low in the atmosphere and to have pursued a nearly horizontal course at heights of 22 to 18 miles. This result assumes the alt. at Mallow and Cleggan to be only half that stated. The radiant was in Aries, or immediately S.W. of that constellation, at about 23° +7°, and therefore close to the horizon, and there is presumptive evidence that this fine meteor may have belonged to the same stream as the fireball seen at Bristol and many other places on November 17, 1886, which had a radiant at about 34° +19°, and moved with the same extreme slowness and intermittent brilliancy*. If the initial alts. of 30° and 25°–30,° estimated at Cleggan and Mallow, are accepted, then the height of the fireball of Nov. 17 last would be about 64 miles over a point near Longford, but this would be quite incompatible with the alts. at Kingstown and Mountrath. The descriptions are so discordant that the adopted height of about 20 miles must be considered provisional and liable to such alteration as further observations may require. There is no doubt the course of the meteor deviated little from an east to west direction, and that it passed nearly overhead at Duleek and Drogheda, for the observers at these places are in exact agreement, and are corroborated by several accounts from the neighbourhood of Cleggan (on the extreme W. coast of Ireland), where the meteor was first seen very slightly N. of East. This fireball is remarkable for its great length of path (certainly more than 250 miles) and for its long duration. Near its disappearance it was evidently projected upon, or distinctly visible through, clouds near the horizon.

Though emanating from much the same region, this brilliant visitor can hardly be associated with the Taurid meteor-shower which, in the month of November, yields many large, and sometimes detonating, fireballs, including those of Nov. 12 and 19, 1861, Nov. 20, 1864, Nov. 21, 1865, Nov. 8, 1876, Nov. 23, 1877, &c. At this season the constellations of Taurus and Aries appear to be the focus from whence a considerable number of the finest meteors are derived.

It is most regrettable that the fortunate observers of the more brilliant forms of meteoric phenomena are usually so remiss in noting important details. From this cause nearly all the fireballs which appear are enabled to pass without satisfactory determination. In November last unusually large meteors were recorded on the alternate nights Nov. 13, 15, 17, 19, and 21; but in every case the observations are either too vague, incomplete, or discordant to afford useful materials for computation.

Bristol, 1887, Dec. 14. W. F. DENNING.

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* See 'Observatory,' No. 119, p. 66.

Selenographical Notes.

Diophantus and Delisle.—Between 8^h and 10^h on 25th November, 1887, the vicinity of these little ring-plains was favourably placed for observation, the morning terminator lying from 1° to 2° east of Delisle, the more northerly of the pair. When the Sun has attained an altitude of about 4° or 5° on the surface immediately north of Diophantus a faint light streak becomes visible, extending from the small crater *b* on the north-west of it to the low white plateau *a* (alpha) on the north of the ring-plain, and terminating a little east of it. At this stage of illumination the marking has an elongated spindle-shaped form, convex towards the north-east. As the lunar day progresses it can be traced much further in an easterly direction, and ultimately up to a little crater south of Diophantus *a*, where it appears to terminate. It is thus shown by Schroeter, Sel. Top. Frag. t. xix. While examining this feature on the above date with powers of 284 and 350 on a 8½-inch Calver-reflector, a cleft was discovered on its site running from the north side of Diophantus *b* to the north boundary of the shadow thrown by the eastern side of Diophantus. At this time no traces were apparent of the object, so conspicuous at a later stage, described and shown by Neison as a plateau (Neison Map IX.), but nearly in the same situation three minute white spots were noted bordering the northern flank of the cleft. Though this latter object is of a very delicate type it was satisfactorily glimpsed on several occasions with 284, and steadily seen with the higher power on a night when definition could only be described as moderately good. On either side of it, even under the oblique illumination which prevailed, traces were apparent of the faint silvery lustre of the spindle-shaped streak, to which the cleft seems to form an axis. We have here another instance, among many, of the association of clefts with faint linear markings of this description. In most of the coarser examples, their wide sloping sides are visible under a high light long after any traces of shadow remain whereby they can be distinguished. In this and other cases, however, the streaks are far too wide to admit of the supposition that they represent the flanks of such contracted furrows, but rather suggest the idea that they are due to the presence of some kind of highly reflective material deposited in the neighbourhood of the clefts when they were formed. On the same evening another rill was suspected running from the most westerly of the three white spots near the site of *a* (alpha) to the eastern side of Delisle. There is evidently something in this position which is not an ordinary ridge. The place of the small crater on the southern wall of Delisle was marked by a dark notch or gap, and a notch on the southern wall of Diophantus pointed to the existence of a similar feature in a corresponding position on the border of this ring-plain, though it is not shown in any of the maps. Strange to say, Schmidt alone draws the large, deep crater a mile or two south of Diophantus, which may perhaps be explained by the fact that, though under a high Sun it is an easy object, for some time after sunrise it is obscured by shadow. There are two remarkable ridges in connection with Diophantus—one running from the foot of the northern wall to the brilliant, triangular-shaped elevation β on the south-east of Delisle; the other extending from near the large crater just mentioned in a south-easterly direction. The first casts a broad, sharply-defined band of shadow, which gradually widens towards the north, and the other an equally sharp but narrower band, like a black line, which disappears soon after sunrise. Both these features may, I think, be classed as steep walls of no great altitude rather than ridges. The straightness and sharpness of the shadows of both are very noteworthy. The crater Diophantus *a* is flanked on the north and south by a smaller crater, and on the west by what seems to be a little incomplete ring, which may be looked for when the E. long. of the morning terminator is about 38° or 39°. Neison refers to a small pass on the south wall of Delisle, which Schmidt does not show. On the 27th November this feature was plainly seen under a solar altitude of about 25°, together with a faint ridge or marking which appears to abut on the ring at this place. At this phase the large central mountain of Delisle, which occupies about a fifth of the area of the floor, is conspicuous, as are also two bright but ill-defined spots about midway between Delisle and the low plateau north of Diophantus.

CARDANUS AND KRAFT.—Schmidt draws a very large crater on the west side of the floor of Kraft, shown neither by Mädler nor Neison. Is the object which is visible in this position really a crater or a slightly curved elevation which, when partially covered by the shadow of the wall, has all the appearance of being the eastern side of a large ring? This was the phase between $8^h\ 30^m$ and $9^h\ 10^m$ on 29th of November, 1887, when anyone observing the formation for the first time would have unhesitatingly recorded a great crater in the interior. When the floor is fully illuminated, however, the only object visible thereon is a large oblong white mountain on the south-western quarter of it. It is perhaps worth recording that the curious cleft which runs between raised banks from Cardanus to Kraft is easily traceable a few hours before full moon.

Kempston, Beds, 1887, December 19. THOS. GWYN ELGER.

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*Stars and Meteorites**.

THE results communicated by Mr. Norman Lockyer to the Royal Society on the 17th of last November are of a nature to modify profoundly current ideas as to the constitution of the universe. It is not possible to escape their influence. They may not all prove unimpeachable; the striking hypothesis by which they are rendered coherent may have to be rejected or modified; but opinion cannot stand where it stood before. A *col* has been crossed, and a new horizon widens out before us. We are bound to survey it with attention.

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* "Researches on Meteorites." By J. Norman Lockyer, F.R.S. 'Nature, November 17 and 24, 1887.

Meteorites offer authentic samples of cosmical matter. Some of them undoubtedly come from interstellar space, and seem, by the possession of hyperbolic velocities, disconnected from the solar *cortège*; so that any one of the specimens ranged on the shelves of our museums may quite conceivably have found its way thither from the confines of the Milky Way. It has been computed that at least one such object is contained in every 210 cubic miles of space; their numbers must hence be prodigious, and there is reason to think that the solar system is at present traversing a region in which they are far less thickly strewn than elsewhere.

The systematic study of their spectra at comparatively low temperatures has, in Mr. Lockyer's hands, proved singularly fruitful. The success so far achieved is largely due to the moderate amount of heat employed, and gives an emphatic sanction to the extension downward, so to speak, of the celestial spectroscopic range of inquiry. The research is still in progress, and the conclusions derived from it are to be regarded as in a measure provisional. Yet already the key has been found to more than one enigma of long standing; facts, apparently discordant, have been coordinated, and an unsuspected bond of relationship has been shown to link together in harmonious sequence the various orders of heavenly bodies.

The proof that nebulæ—those, at any rate, giving a gaseous spectrum—are closely allied to comets may be said to be complete. That comets are formed of meteoric materials is universally admitted; it now appears that the whole of the seven bright lines derived from nebulæ can be obtained in the laboratory by gently heating fragments of meteorites in a glass tube through which an electric current is made to pass. Of these three are familiar hydrogen-lines, three are due to "cool" magnesium. Only the seventh, at wave-length 495, remains unexplained; and since it has been seen in the glow of the Dhurmsala meteorite, it also may be expected before long to yield up the secret of its origin.

"The result of this comparison," Mr. Lockyer remarks, "is that the nebula spectrum is as closely associated with a meteorite glowing very gently in a very tenuous atmosphere given off by itself, as is the spectrum of a comet near the sun with a meteorite glowing in a denser one also given off by itself when more highly heated."

The fundamental nebular line—that at wave-length 500—is the chief constituent of the light of magnesium-vapour at the temperature of the Bunsen burner; and it is associated with an ultraviolet line at 373, photographed by Dr. Huggins in 1882 in the spectrum of the Orion nebula. A line at 470, also belonging to magnesium, has been observed in three nebulæ—Webb's in Cygnus, Copeland's double object, and the planetary No. 4964—and was also apparently measured by M. von Konkoly in the spectrum of

Comet 1881 III.* As a rule, however, magnesium is represented in comets (when represented at all) by the solar group *b*, which, as the temperature rises, completely supersedes the "remnant of a fluting" at 500. It is hence inferred that comets are usually much hotter than nebulæ; and, as if to emphasize this conclusion and bridge the gap between the two classes of bodies, we meet with trustworthy observations of the distinctive nebular spectrum in a pair of comets which, throughout their elliptical revolutions, maintain a respectful distance from the Sun. Dr. Huggins found the only discontinuous part of the emissions of comet 1866 I. (of the November meteors) to be situated in the precise position of the chief nebular line; and the light of comet 1867 II. proved to be of the same quality †. It is peculiarly fortunate that a fact, unlikely to be frequently conspicuous, should have been thus early placed on record.

The evidence of a close constitutional analogy between nebulæ and comets is thus so strong that the total absence of spectroscopic indications of carbon in the one case or of hydrogen in the other occasions some perplexity. Both substances (the former in a state of combination with oxygen or hydrogen) are, it is needless to say, largely occluded by meteorites. When heated in the laboratory they display by turns the lines of hydrogen and the flutings of carbon; but the flutings can always be developed at the expense of the lines by raising the temperature. The exclusive appearance of either one or the other seems to depend somehow on the conditions of illumination. That hydrogen enters into the composition of comets can scarcely be doubted; it is almost equally safe to affirm that carbon enters into the constitution of nebulæ.

Stars showing bright lines in their spectra are among the most interesting of sidereal objects. They vividly illustrate the unity of the stellar and nebular schemes, conducting us by almost insensible gradations from the poignant lustre of a true star to the soft sea-green glimmer of a planetary nebula. In one noted instance the transition has been visibly effected as the simple consequence of a rapid running-down of temperature; and Mr. Lockyer does not hesitate to draw, from the "migrations" of Nova Cygni, the inference that all bright-line stars are in reality hotter, because more condensed nebulæ. Their spectra, like those of nebulæ and comets, integrate, he asserts, the emissions of innumerable solid particles, of the individual vaporous atmospheres surrounding them, and (where hydrogen-lines are present) of the tenuous gases diffused through the "interspace" separating them.

Such a constitution, however, appears incompatible with radiations of a high order of intensity; yet γ Argûs, γ Cassiopeiæ, and β Lyræ are brilliant stars of the second and third magnitudes. The light we receive from each of them certainly very far exceeds what we should receive from our Sun at their unmeasured and probably immeasurable distances; the copious and perennial flow of their luminous emissions argues the possession of even vaster reserves of heat than the solar interior holds; nor is it conceivable that their contained supplies could by any machinery less effective than that at work in the Sun be brought with sufficient rapidity to the surface. On this ground alone, then, we are obliged to regard them as "suns," that is, as masses of incandescent vapour sheathed in photospheric clouds; and the strongest confirmation of their sun-like nature is afforded both by M. von Gothard's observation of the three hydrogen-lines *dark* in β Lyræ, September 5, 1882 [*], and by the presence in the same object of a brilliant D_3, certified beyond question by Vogel's measurements with the great Vienna refractor, June 2, 1883 [†].

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* 'Beobachtungen in O'Gyalla,' Bd. iv. p. 6.

† Proc. R. Soc. vol. xv. p. 5; Month. Notices, vol. xxvii. p. 288.

The meteoric relations nevertheless of bodies of this kind, more especially of those of the γ Argûs type, have been established by the detection of several of their characteristic, and until now enigmatical, bright lines in the laboratory-glow of meteorites. All the facts would, we believe, be harmonized by supposing electrical discharges of the nature of terrestrial auroræ to occur in stellar atmospheres charged with meteoric dust and vapours. Nothing is more certain than that such materials must enter as ingredients into the gaseous surroundings of all the heavenly bodies; and those of bright-line stars, owing to their vast probable extent (to say nothing of other circumstances), should receive peculiarly large meteoric supplies. What we may venture to call the auroral illumination to which the bright lines are due may even be excited and maintained by continual fresh infalls, just as our Northern Lights are set flashing by shooting-stars. The mode of production here suggested, by electrical discharges high up in the stars' atmospheres, would only account for "low-temperature" lines. Stars showing D_3 bright should be relegated to another category; and it may be anticipated that their spectra, when they come to be photographed, will include several of the ultra-violet hydrogen series discovered by Dr. Huggins.

Besides variables emitting lucid rays at maximum, nineteen bright-line stars are at present known. Of these no less than eight are grouped about the remarkable bifurcation of the Milky Way in Cygnus, and all (with the solitary exception of β Lyræ) are involved in the galactic stream. That their peculiarities are connected with their situation in space can thus admit of no rational doubt; and the influences affecting them are now shown to be of the same character with those bringing about the stellar outbursts of which the Milky Way is the favoured scene. Since these are almost demonstrably meteoric it appears to follow that the Milky Way is a region of condensation for meteor-swarms as well as for stars; and the enormous distance from our system of most, if not all, gaseous and temporary stars may be inferred from

[*] Astr. Nach. No. 2581.

[†] 'Publicationen,' Potsdam, No. 14, p. 15.

their association with the remote annulus girdling our skies with a dim track of light.

The spectroscopic history of Nova Cygni curiously illustrates the mutual relations of sidereal species. At the height of its incandescence it might be classed with β Lyræ; as its temperature fell, it emitted rays common to γ Argûs and the Swan-stars, lapsing finally into the "cool magnesium" stage of a planetary nebula. It is now believed to give ordinary stellar light, but wears at times the hazy aspect occasionally put on by a few peculiar variables and effete "Novæ" *.

In each of its successive phases meteoric connections are so evident as to leave little doubt about the agency by which the blaze of November 24, 1876, was kindled. Mr. Lockyer explains the catastrophe by the clash of two oppositely-moving meteor-swarms; but the example of T Coronæ teaches us that ordinary stars are subject to similar conflagrations, due, no doubt, to their becoming suddenly overwhelmed by a rushing cosmical cloud. The mystery attending the appearance of "new stars" is, at any rate, to a large extent dissipated. Their outbreaks merely exaggerate enormously the phenomenon of the Earth's encounter with a drift of Leonid or Andromede meteors. The extension of the same rationale to other kinds of stellar variability is inevitable; nor can the periodicity of sun-spots be ranged under a separate heading. No surer proof could be alleged of the effectiveness of the research being prosecuted at South Kensington than that it affords the means of grouping together such diverse appearances as effects of a common cause, and that one so seemingly insignificant that it might well be taken to be of small account in the universal economy.

The spectrum of R Geminorum—a remarkable variable displaying a range of bright lines at maximum—may be called purely meteoric. Its experimental reproduction from the vacuum-glow of meteorites is a triumph of sagacious reasoning and patient investigation. A notable advance has at the same time been made in the interpretation of the "flutings" visible in the analyzed light of stars of Secchi's third type. They are shown to be in part due to the *direct* radiations of carbon, in part to the absorptive action of zinc and manganese. Nevertheless, and in spite of the pair of bright carbon-flutings, we must demur to classing so lustrous an object as α Orionis with comets and such-like meteoric aggregations as a "cloud of incandescent stones." Professor Pickering's photographs of the spectra of Mira Ceti and Nova Orionis supply information worthy of careful study as to the nature of this species of star.

Besides a multitude of dusky bands and channellings, six lucid hydrogen-lines are apparent in them. These are G, *h*, and four of the "white-star" set, emitted, we know, only at a high pitch of thermal excitement. But the intermediate H-line is absent, the gap being filled by the calcium H and K, distended and obscure as in the solar spectrum. The obvious inference is that the glowing hydrogen giving the *bright* lines is overlaid by the vapour of calcium giving the *dark* ones; the ray missing from the hydrogen-series is stopped out because of the approximate coincidence of period between the vibrations producing it and those of the superposed absorbing substance. The stratum of hydrogen kindling into a blaze in these stars with the periodical increase of their light must then be situated relatively near their photospheres, while metallic vapours at lower but still very high temperatures are piled up above, and exact toll of all the radiations transmitted across them. There is indeed strong reason to attribute to stars showing banded spectra, whether of the third or of the fourth type, enormously diffused gaseous surroundings, including a great variety of heat-levels; and with this peculiarity the instability of light predominant among them can readily be associated.

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* J. G. Lohse, Month. Notices, vol. xlvii. p. 494.

The general conclusions arrived at by Mr. Lockyer may be thus summarized. All self-luminous bodies in space are composed of meteorites variously aggregated and at various stages of temperature depending upon the frequency and violence of their mutual collisions. Comets, nebulæ, bright-line stars, stars showing banded spectra of the third type, including most long-period variables, are to be regarded as veritable meteor-swarms—they are made up, that is to say, of an indefinite multitude of separate and, in a sense, independent solid bodies, bathed in evolved gases, and glowing with the heat due to their arrested motions. "The existing distinction," we are told, "between stars, comets, and nebulæ rests on no physical basis." Stars, on the other hand, of the Sirian and solar types (constituting the only true "suns") are vaporized meteor-swarms; their high temperatures represent the surrendered velocities of myriads of jostling particles, drawn together by the victorious power of gravity.

"Collisions" are not, however, exclusively relied upon for the cosmical production of light and heat. It is admitted that the luminosity of comets and nebulæ must be largely due to electrical excitement; nor is any reason apparent why its influence should be restricted to these two classes of bodies. Destruction of movement by impacts can scarcely be made to supply its place. Occasional illuminative effects may be derived from it, but none that are uniform and permanent.

The small solid bodies which, more or less plentifully distributed, appear to pervade space, are in this theory treated as the fundamental atoms of the universe. But it is evident that we cannot begin there. They have a history, marked perhaps by strange vicissitudes. They may be agents of regeneration, but they are almost certainly products of destruction. Possibly they are seed as well as dust, and serve as the material link between the creation and decay of successive generations of suns. A. M. Clerke.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

[The following letter was kindly written by Dr. Gill in response to a request made to him by us for periodical intelligence from the Cape. We shall be very glad to receive news from other, especially Colonial, observatories, such as might not suitably appear in official communications, but might still be of considerable interest to the astronomical world.—Eds.]

The Cape Observatory.

Gentlemen,—

You ask me to write, for publication in the 'Observatory,' some astronomical news and gossip from the Cape. I do so with great pleasure; not that there is much to tell in the way of scientific news, for the mills of Astronomy grind slowly, and stellar parallax, fundamental declination, a photographic Durchmusterung, and geodetic survey are not things that present a very changeful nor, to some people, a very amusing form of gossip; and of astronomical politics we have none.

But there is a good deal of wisdom in the saying attributed to a German coleopterist—"Mankind is subdivided into two classes, those men who collect beetles and those men who do not collect beetles; for myself I am only interested in the smaller but more important of these two subdivisions of mankind, namely the first." Without taking up quite so sharply defined a classification of mankind as this, I agree entirely with you when you say that modern facilities of travel have introduced so many astronomers to each other, that personal intelligence about astronomers, about their work, their observatories, and their daily doings may be of considerable interest to many of your readers; at least I feel, for my own part, that such news of many good astronomical friends would interest me very much. Therefore, in the hope of receiving like news of them through the medium of the 'Observatory,' I will endeavour to comply with your request.

Well, the good steamer 'Tartar' brought my wife, the new heliometer, and myself safely back to the Cape; and you may be sure that no time was lost in erecting the instrument in its new home.

In a future letter I will send you some account of this instrument and its observatory. Both have many novel features, and both have proved very satisfactory in practice.

Active working with the heliometer was much delayed by the non-arrival of the accumulators requisite for illumination of the scales, circles, &c. of the instrument, for all the readings were arranged for electric illumination only. Batteries were tried, and proved very troublesome, failing often in the middle of an observation. But at last the accumulators came to hand, and since their arrival no failure has occurred. But the charging of accumulators by Grove batteries is a troublesome, dirty, and expensive business; and I am happy to say that Treasury sanction has been obtained for the purchase of a dynamo (and the other necessary plant), to be worked by one steam-engine; and so the battery difficulty will be got rid of.

The heliometer is being exclusively devoted to researches on stellar parallax, on the lines which have been already indicated—that is to say, more with a view to the determination of the parallax of a considerable number of stars, each with a considerable but not exceeding high accuracy, as what we want to know is rather the average parallax of a star of the 1st, and 2nd, and 3rd magnitude than what is the precise parallax of a particular star. A programme has been prepared, which will cover about five years' work; the necessary comparison-stars have been selected, and the instrumental settings and other necessary particulars computed; but it will be some time before I can have much to tell about results.

We have no comets on hand just now. Now and again a little excitement is created by Mr. Finlay finding another cometary-looking object not in Herschel's list of nebulæ; but it always proves to be a true nebula, though not recorded by Sir John. Mr. Finlay has now quite a list of such objects, all picked up with a telescope of six inches aperture. But we are in the midst of a longitude operation, viz. Cape—Port Elizabeth. This work will conclude the field operations of the first part of the Geodetic Survey, and I am forming a computing office for the reduction of this work, and hope to include the reduction of the Tidal Observations. The base-line measurement at Port Elizabeth (an operation in which I took part shortly before my departure for England) shows that the length of that base (3 miles) differs by 2 inches from the length of the same base when computed from the Natal base through the intervening chain of triangles. Of course this results from only a preliminary reduction, and without any adjustment of the angles beyond the correction of the sums of the angles of each triangle to 180°+spherical excess, but it is sufficient to show the general reliability of the work. The distance between the two bases is between 400 and 500 miles.

Like all geodetic operations that I know, our work is deficient in Astronomical Stations. The local deviations of gravity and the errors of astronomical results generally bear too great a proportion to the errors of the linear measurements, and, properly speaking, latitude and azimuth should be determined at every station, but the expense would be too great. There are, however, a sufficient number of astronomical stations to give the work considerable geodetic interest.

Major Morris, R.E., will take up his quarters with us about Christmas, after some years of very hard and excellent work in the field, and the work of reduction will be pushed to completion as quickly as possible.

The Photographic Durchmusterung is proceeding rapidly, the instrument being kept at work by two observers from evening twilight till dawn. The reduction of the plates from S.P.D. 0° to 12½° has been completed by Professor Kapteyn, and he has the plates on hand for measurement to S.P.D. 30°. Upon him is really falling the hardest work of this undertaking, and he is facing it with the greatest energy, in order that the Catalogues may be available as soon as possible. Derby dry plates are now being used with an exposure of half an hour instead of an hour, as required for like results with the plates formerly employed.

Royal Observatory, Faithfully yours,
Cape of Good Hope. D. Gill.
1887, Nov. 30.

The Periods of the Planets.

Gentlemen,—

I read with great interest the Editorial note on my letter ('Observatory,' No. 129, p. 391), and am indebted to you for having drawn attention to the doubts which must arise from a comparison of the law of multiples with the period.

I cannot deny that in the case of planets the greatest differences for Mars and Saturn amount to $\frac{T}{4\cdot5}$ and $\frac{T}{6}$ respectively, though the least differences for the Earth and Jupiter are only $\frac{T}{252}$ and $\frac{T}{54}$. While these differences nearly attain the theoretically admissible limits of error $\frac{T}{4}$ in one case and in numerous other cases are nearly vanishing is, I know, rather difficult to explain; but it appears to me that it could be partly accounted for in the case of Uranus and Neptune by the uncertainty of their periods, and in the case of Mars and Saturn by the large mass of Jupiter. Indeed the electrodynamic laws, by consideration of which the present law was found, do not apply exactly to our solar system, inasmuch as the large masses of Jupiter and Neptune must greatly disturb the motions of Mars and Saturn.

The idea occurred to me that the cause of the large differences may be found in the disturbances, if not only two but three or four bodies act one on another. The near approach of Mars to Jupiter, the largest planet, causes therefore the greatest difference; and the next considerable difference with Saturn. Jupiter being a superior planet in respect to Mars and inferior with respect to Saturn, its disturbing action is, it seems to me, duly represented in the formula by the change of sign; the same holds good for Uranus to which Neptune is superior, and for Venus to which the Earth is superior. The negative sign in the case of Neptune seems to indicate a planet of great mass still more remote. It is obvious that the law can only be approximate and not exactly applicable to intensely disturbed planets, satellites, periodic comets, and meteor showers, which must differ from the formula in proportion to the amount of disturbance they sustain in their path. I believe this may account for the considerable discrepancies in the tables for planets and satellites, showing apparently that in the case of satellites the action of the Sun cannot be neglected; much less, therefore, the action of Jupiter and Neptune on the neighbouring planets.

Faithfully yours,
Ch. V. Zenger.

[We only print this letter in deference to the author's right of reply; and must disclaim any sympathy with the speculations it contains. We hope that in making one or two verbal alterations we have not further obscured the author's meaning.—Eds.]

The Rotation of Jupiter.

Gentlemen,—

Mr. Lynn ('Observatory,' No. 130, p. 431) supports Prof. Hough's opinion that the red spot is probably "a portion of the actual body of Jupiter," and that the variable rotation period it shows may really affect the entire sphere of the planet. In reference to these views I should like to mention that it seems far easier to consider the red spot an atmospheric phenomenon than that the planet is subject to remarkable fluctuations in its rotation period. Undoubtedly the spot is a very durable feature, but we have only been certainly cognizant of its existence during the last 9 years, and I see no difficulty in accepting the theory that Jovian phenomena may be of such a nature as to sustain an atmospheric object during many consecutive years. The probably heated surface of Jupiter is involved in dense gaseous vapours, which quite possibly develop forms capable of resisting elimination over extended periods. The telescope has unmistakably taught us that every object on the planet is subject to changes in appearance, and that certain lineaments, though very persistent, ultimately collapse, to be supplanted by others.

There is no significance in the fact that the rotation derived by Mädler and Airy in 1835 is less than the present period of the red spot. Nor is it feasible to assume a lengthening rotation from these data. More than two hundred years ago, Cassini gave the period as $9^h\ 55^m\ 50^s$, and for an equatoreal spot $9^h\ 50^m$, which is but slightly different to the rates exhibited in our own time. In 1708 Maraldi gave $9^h\ 56^m\ 48^s$, in 1779 W. Herschel $9^h\ 55^m\ 40^s$, in 1786 Schroeter $9^h\ 55^m\ 33^s{\cdot}9$ and $9^h\ 56^m\ 56^s$, and in 1866 Schmidt $9^h\ 55^m\ 46^s{\cdot}3$, so that the planet's period has certainly not increased. Different spots give different periods, and the tendency of individual markings is to slacken their velocity during the time of visibility. Scarcely any two spots will be found to travel at the same rate, unless they are situated in the same longitudinal current. There were spots visible in the autumn of 1880, which completed a rotation in only $9^h\ 48^m$, and there are others which, like the red spot, approach $9^h\ 56^m$.

If the rotation periods deduced from the earliest times gave us the slightest indication of an increasing period, we might venture upon the hypothesis that the body of Jupiter participated in the changes observed; but there is really no evidence to favour such a view. The discrepancies between the various observations are adequately explained by the numerous longitudinal currents operating on the disk, which doubtless give rise to the belted forms and occasion many alterations in the appearances and in the relative places of certain markings. In some cases these currents may overlie each other, and thus occasion temporary obscuration in the objects at lower levels. In nearly the same latitude as the red spot there are some faint grey belts which make circuits of the planet in 18½ sec. less time than the spot (see 'Observatory,' No. 110, p. 191), and are sometimes seen either overlying or underlying that object.

Moreover the lengthening period of the red spot has not been *regular*. During the three years preceding March 1882 it increased 4 sec., whereas during the three following years the rate remained stationary at $9^h\ 55^m\ 39^s{\cdot}1$. In 1885–86 it further increased 2 sec., while during the last opposition (1886–87) it decreased $0^s{\cdot}6$. These vagaries can hardly represent the behaviour of the "giant planet" himself. They are sufficiently accounted for on the supposition that the spot is a formation quite detached from the Jovian sphere and affected by turbulent action in surrounding objects, each of which may constitute a focus of disturbance. We are ignorant as to the origin of the different forces exerting themselves so obviously upon the planet; but they possibly result from the evolution of heated material from the body of the planet and the action of the rapid axial rotation upon them. Some of the gaseous eruptions may, on entering the lower temperature at considerable elevations, be contracted into tolerably compact masses, and thus enabled to preserve their integrity of outline during long intervals. The red spot perhaps owes its existence to phenomena of this kind and may continue visible for some years yet, though not necessarily a part of the surface, but rather floating above it amid the numerous other curious forms that diversify its atmosphere.

Bristol, 1887, Dec. 7.

Yours faithfully,
W. F. Denning.

The Council of the R. A. S.

Gentlemen,—

I venture to think that it would have materially added to the value of the list printed on p. 438 of the 'Observatory' for the current month, had its compiler supplemented his bare enumeration of the years of service of each of our present Officers and Council by a list of their possible and actual attendances at our Council Meetings during (say) the past Session. It by no means follows that he who has served the Society longest has served it least, and it might materially help the Fellows in their selection of the executive body if they were informed who the men are who really do the work, in contradistinction to those whom we may regard as merely ornamental members of that body. For myself, I would rather get rid of any one who has figured nominally only on the Council for one year, than I would of a member who could point to twenty-five years of assiduous attendance and real personal work on it.

Faithfully yours,
William Noble.

Forest Lodge, Maresfield, Uckfield,
1887, Dec. 14.

[The desired information shall be given next month.—Eds.]

The Olbers-Brooks Comet.

Gentlemen,—

Observations of this comet, discovered by me on the morning of 1887 August 25th, have been made with the 9-inch silver-on-glass reflector on every available opportunity. The work has been greatly interrupted by cloudy weather and moonlight. I send herewith a few notes and sketches.

At discovery the comet was an easy object in the 9-inch reflector. The nucleus was brightish, though diffused, and the existence of a short faint tail was noted. Four days afterwards, *i. e.* on August 29, the comet presented the appearance shown in fig. 1.

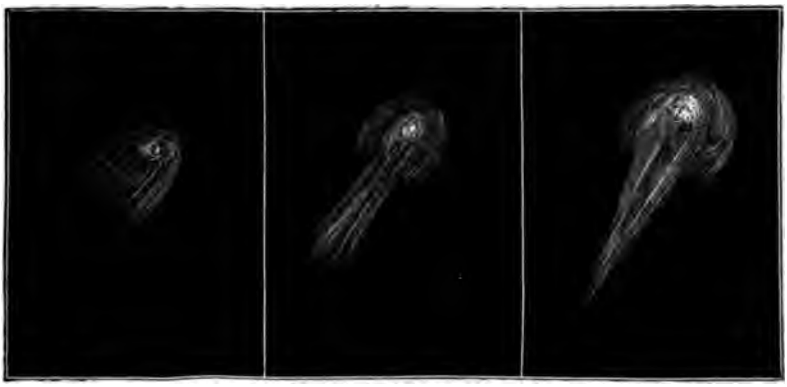


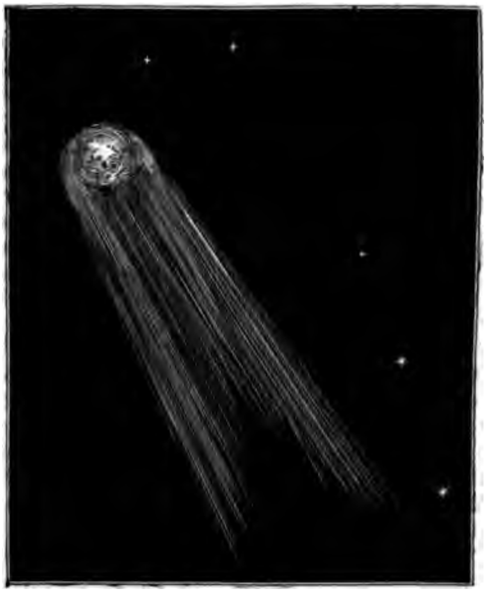
Fig. 1.—Aug. 29. Fig. 2.—Aug. 31. Fig. 3.—Sept. 17.

Telescopic Views of the Olbers-Brooks Comet, 1887. Power 100.

Aug. 31.—Comet was much changed in form, as indicated in fig. 2, and was just visible in a 2-inch achromatic of short focus, mounted on an equatoreal stand with circles as a star-finder.

Sept. 17.—Comet was found considerably brighter, and the tail much more prominent. Its appearance is shown in fig. 3.

Fig. 4.—Oct. 14.



Olbers-Brooks Comet. Power 100.

Fig. 4 shows the appearance of the comet on Oct. 14, about six days after perihelion passage. This was probably the epoch of its greatest brilliancy. The tail was broad, and the nucleus decidedly granular in appearance when viewed with powers of 100 to 150 diameters. Its whole appearance was that of a large naked-eye comet in miniature.

Yours faithfully,
William R. Brooks.

Red House Observatory,
Phelps, N. Y., U. S. A.
1887, Nov. 21.

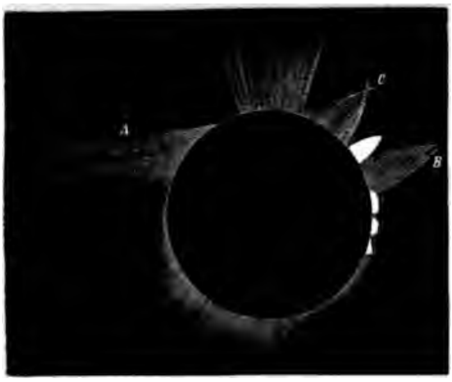
[We have also received a letter from Mr. W. H. S. Monck with reference to Prof. H. A. Newton's paper on the "Origin of Comets" (see 'Observatory,' No. 21, p. 310). Mr. Monck has formed a table similar to that diagrammatically represented on p. 320, and gives an account of the result, which on examination we take to be coincident with Prof. Newton's.]

NOTES.

The Total Solar Eclipse of 1887, August 19.—Dr. B. von Engelhardt has kindly sent us a German translation of a Russian paper by Dr. Handrikof, Professor of Astronomy at Kief, containing an account of his observations of the above eclipse from Mount Blagodat, in the Urals. He was armed with a 3½-inch refractor, a sextant, and a chronometer; and besides observing the four contacts and occultations of spots, he made four drawings of the corona, which agree essentially with the photographs taken at Krasnoiarsk. They are summarized in the subjoined woodcut taken from the original paper, the white parts representing prominences. A long group of prominences also appeared at the base of ray A towards the end of totality. In fact the prominences were peculiarly striking and numerous, contrasting strongly with the recent scarcity of spots; and from this fact and the shape of the corona, especially the tangential direction of ray A, Dr. Handrikof feels entitled to draw the following remarkable conclusions:—

1. There is no particular connection between Sun-spots and protuberances—at least not the connection claimed by M. Faye in his theory of the solar constitution.

2. The corona is not material, but is an optical phenomenon, perhaps originating at the Moon's surface; and reaching our eyes owing to the Earth's atmosphere ("welche vielleicht an der Mondoberfläche stattfindet, und unserem Auge durch die Vermittelung der Erdatmosphäre zugeführt wird").



SPECTROSCOPIC STAR-CATALOGUES.—The eighth volume of the O'Gyalla publications contains a Catalogue of the spectra of all stars to the 7½ mag. included between the zones 0° and −15° declination. Prof. Vogel of Potsdam laid the foundation of such Catalogues in his Spectroscopic Durchmusterung of the zone 0° to +20° declination, and this was followed by Duner's Catalogue embracing the zone +20° to the N. Pole. The three Catalogues thus give us a complete register of the spectra of stars in the whole of the region from the North Pole to 15° south of the Equator. From the time of Flamsteed great exertions have been made to form star catalogues, whereby the positions and motions of stars can be deduced. Could not some Southern Observatory set about completing the Spectroscopic Catalogue? We should then have a starting-point for detecting physical changes in the Universe, in the same way in which we can now detect changes of position. The present work was begun in August 1883, and completed in the summer of 1886, 2022 stars in all being examined. The observations were made with a Zöllner star spectroscope when the star was near the meridian. Vogel's types have been taken as the standard.

Mr. Lockyer's Meteoritic Theory.—In the 'Comptes Rendus' of Nov. 28, M. Stanislas Meunier objects to Mr. Lockyer's hypothesis, that meteorites are made up of metals whose melting-points are so different, that the agglomeration cannot be supposed to have occurred at a high temperature. He contends that the only conclusion we are as yet entitled to draw from the spectroscopic researches on meteorites is that they are composed of the same original matter as other celestial bodies.

The Nautical Almanac.—The volume for 1891 is now published. The only alterations made in the general arrangement are that the angles for occultations visible at Greenwich have been reckoned from the true N. point and Vertex towards the east (a system adopted for the last two years in the 'Companion to the Observatory'), and that the following stars have been added to the previous list:—

Star.	Mag.	R.A.	Dec.	Star.	Mag.	R.A.	Dec.
		h m	° ′			h m	° ′
α Phœnicis	2	0 21	−42 54	γ Crucis............	2	12 25	−56 30
ζ Ceti.................	3	1 46	−10 52	γ Centauri.........	2·3	12 36	−48 22
ε Cassiopeiæ	3	1 47	+63 8	β Crucis............	3	12 41	−59 6
γ Andromedæ ...	2	1 57	+41 48	θ Centauri.........	3·4	14 0	−35 50
θ Eridani	3	2 54	−40 44	α Lupi	3	14 35	−46 55
β Aurigæ	2	5 52	+44 56	β Boötis............	3	14 58	+40 49
ζ Canis Maj.	2·3	6 16	−30 1	β Trianguli Austr.	3	15 46	−63 6
δ Canis Maj.	2	7 4	−26 13	β Aræ	3	17 16	−55 26
ζ Argûs	2	8 0	−39 42	θ Scorpii	2·3	17 29	−42 56
ε Argûs	2·3	8 20	−59 10	ε Sagittarii	3·2	18 17	−34 26
δ Argûs	2·3	8 42	−54 19	σ Sagittarii	2·3	18 49	−26 26
λ Argûs	2·3	9 4	−43 0	α Indi	3	20 30	−47 40
β Argûs	2	9 12	−69 16	α Delphini	3	20 35	+15 32
κ Argûs	3·2	9 19	−54 33	γ Pavonis	4·3	21 17	−65 52
θ Argûs	2·3	10 39	−63 49	δ Capricorni	3	21 41	−16 37
μ Argûs	3	10 42	−48 51	α Toucani	3	22 11	−60 48

There will be a transit of Mercury on 1891, May 9, partly visible at Greenwich; but the eclipses are not very interesting.

Occultations of Stars by Planets.—In the 'Astr. Nach.' No. 2814, Herr Berberich gives a list of possible occultations of stars by the planets Venus, Mars, Jupiter, and Saturn during the year 1888. He calls attention to the great importance of observations of such phenomena, and the scantiness of the information hitherto accumulated on the subject. The following calendar may be useful to observers, though we must refer them to his original paper for details of the individual phenomena.

G.M.T.	Planet.	Star's Mag.	G.M.T.	Planet.	Star's Mag.	G.M.T.	Planet.	Star's Mag.
d h			d h			d h		
Jan. 5 17	Venus	9·7	May 2 11	Jupiter	8·8	Oct. 20 23	Venus	9·4
9 18	,,	9·5	8 19	Saturn	9·5	22 14	,,	9·1
12 4	Mars	9·3	9 15	Jupiter	10	23 13	,,	9·5
12 9	Venus	9·3	19 15	Saturn	8·8	25 3	,,	9·5
14 9	,,	9·5	23 7	Jupiter	8·0	25 23	,,	9·6
15 2	,,	9·5	June 2 8	,,	8·3	27 13	,,	9·5
17 23	,,	9·3	3 22	Saturn	9·5	28 1	,,	9·3
18 21	,,	7·8	7 14	Mars	9·1	Nov. 1 12	,,	9·5
20 1	,,	9·8	9 1	Saturn	9·5	9 16	,,	9·0
20 20	,,	5·0	9 23	Jupiter	6·3	16 11	,,	9·0
21 3	,,	9·3	July 3 17	Mars	10	16 21	,,	8·3
24 11	,,	9·0	4 8	,,	9·8	20 0	,,	9
24 23	Mars	var.	16 23	,,	9·4	26 23	,,	7·5
25 11	Venus	9·7	25 1	,,	9·6	28 5	,,	8
25 19	,,	8·5	27 14	,,	9·8	29 9	,,	7·8
26 16	,,	9·5	Aug. 3 11	,,	9·4	Dec. 10 13	,,	9·4
31 18	,,	9·3	5 2	,,	9·6	11 15	,,	9·4
Feb. 4 20	Mars	9·9	7 15	Jupiter	7·7	12 0	,,	8·5
5 11	Saturn	9·5	11 6	Mars	9·5	12 6	,,	var.
8 2	,,	9·5	13 8	,,	9·1	12 13	Saturn	9·3
16 7	,,	9·5	15 21	,,	9·8	12 23	Venus	6·0
Mar. 1 6	,,	9·5	16 7	,,	8·8	13 17	,,	9·0
2 4	Jupiter	9·9	17 21	Mars	8·5	13 17	,,	9·4
11 21	,,	9·8	Oct. 1 2	Venus	8·5	15 6	,,	9
23 15	Mars	9·5	4 3	,,	9·5	18 14	,,	9·1
24 15	,,	9·7	4 23	,,	8·0	18 16	,,	9·8
Apr. 12 5	,,	9·9	14 6	,,	9·7	23 10	,,	9·5
13 3	,	9·2	17 14	,,	10	25 12	,,	9·5
13 16	Jupiter	10	18 8	,,	9·1	30 11	,,	9·7
30 17	,,	9·1	19 7	,,	8·7	31 2	,,	9·3

The author also gives some useful formulæ and tables for the reduction of the observations, and remarks that even though there may be no occultation, the near approach of the planet, and especially of its satellites, to the star affords opportunity for important micrometric observations.

Cape Observations.—The results of the meridian observations made at the Royal Observatory, Cape of Good Hope, from 1882 Jan. 1 to 1885 Feb. 8, have now been published, completing the series available for the formation of a Cape Catalogue for 1885, of which the fundamental stars of Schönfeld's 'Durchmusterung' will form the most important part.

The present volume contains 543 pages, and gives tables of instrumental errors, thermometer readings, &c., followed by star ledgers (with means) and catalogues for each year. The number of stars in the Catalogue for 1882 is 863; for 1883, 444; and for 1884 (when a large number of stars were observed by reflection), 1301, counting separately stars observed by reflection or below pole.

In the introduction to the volume, Dr. Gill discusses carefully the errors of the micrometer-screws of the Transit Circle, and refers to many interesting points in connection with meridian observing. Considering the similarity in construction between the Cape and Greenwich Transit Circles, it is curious to find several well-marked differences in their behaviour.

The flexure of the Cape telescope as determined by the horizontal collimators is quite sensible, amounting to $0''\cdot46$, while that of the Greenwich telescope has been for some years nearly zero. On the other hand, the correction for R — D at Greenwich has, since the new mounting of the collimators in 1884 made it possible to extend the observations to Z.D. 70°, shown a steady accordance with the formula $a + 1''\cdot2$ sin Z.D. (where a is a small quantity due to outstanding nadir corrections), indicating a horizontal flexure of $1''\cdot2$; while the R — D at the Cape is "incompatible with the usually adopted law of flexure f sin Z.D., where f is the horizontal flexure determined by the horizontal collimators." The contrast is thus doubly remarkable; and it is certainly curious if the different piercing of the central cube for observation of the collimators, which seems to be the only point of construction affecting the question, is responsible for such a marked difference in the working of the instruments.

We should be inclined to doubt the advantages of the method described on p. 25 for adopting clock-rate from observations at short intervals, even though made by the same observer. It appears to us to reproduce the errors of the tabular places of clock stars, instead of obtaining fundamental places: and also to introduce large accidental errors which could be avoided.

The radical sign is misplaced on p. 31. There seems to be something curious about the adopted level error for Jan. 11 on p. 7; and on pp. 142 and 502, ι' Aquarii should be ι'' Aquarii.

CHANGE OF PERSONAL EQUATION.—In No. 2815 of the 'Astr. Nach.' Dr. Hilfiker gives an account of a change which has taken place in the difference between his personal equations when observing with the "eye and ear" and chronographic methods. This difference has decreased from $+0^s\cdot08$ in 1883 or 1884 to $+0^s\cdot02$ in 1887. It is possible that the decrease is the result of the observer's attention being called to the discrepancy. A very similar series of observations has been conducted at Greenwich with the following result:—

Year of observation.	Excess of clock slow by the Eye and Ear method. H.T.	A.D.	T.	L.	H.
	s	s	s	s	s
1885........	−0·02	+0·06	+0·04	+0·10	+0·63
1886........	+0·05	+0·08	+0·05	+0·12	+0·59
1887........	+0·03	+0·05	+0·02	+0·12	+0·58

There does not seem to be any marked change in any of these instances.

The initials H.T., A.D., T., L., and H. are those of Messrs. Turner, Downing, Thackeray, Lewis, and Hollis.

The Planet Venus.—The striking brilliancy of this planet in the dark mornings of December, coupled with the bold predictions of a few sanguine people that Tycho Brahe's star would again blaze up during the period 1880–1890, have caused a widespread belief that Venus is really the "Star of Bethlehem."

More than 70 letters of enquiry have already (Dec. 28) been received at the Royal Observatory, Greenwich, though the article in the 'Standard' of the 17th inst. has produced a sensible effect in diminishing the daily budget. Perhaps we may draw the attention of those interested in the subject to Prof. Pritchard's letter in the 'Standard' of Dec. 20, where a theory entirely novel, and independent of astronomical considerations, is broached.

New Observatory.—In the Ast. Nach. No. 2816, Dr. N. Herz gives an account of the new Observatory of Herr M. von Kuffner in Vienna, the building of which has occupied some three years. The instruments are a transit-circle by Repsold, of 4·8 in. aperture and 5 ft. focal length, of which the eye end and object-glass can be interchanged, with a circle of 22 in. diameter, read by four microscopes; and a refractor, also by Repsold, of 10·6 in. aperture, and 12 ft. 6 in. focal length, with position, double-image, and ring-micrometers, besides smaller instruments and apparatus. A prime vertical instrument is to follow. The latitude of the transit-circle is provisionally determined to be 48° 12′ 47″·2 and the longitude $1^{h}\ 5^{m}\ 11^{s}\cdot 1$ east of Greenwich.

Recording Apparatus for the Wedge Photometer.—Attention has already been called to the desirability of attaching some sort of recording apparatus to this instrument, whereby several readings may be registered without disturbing the condition of the eye by bringing up a light to read the micrometer head. Prof. Young especially mentions this point in his report on the wedge photometer in the 'Annals of the Harvard College Observatory,' vol. xviii. No. 2, Herr E. V. Gothard, in the 'Zeitschrift für Instrumentenkunde,' describes a simple apparatus which he has devised for this purpose and which apparently satisfies all requirements.

Osservazioni di Stelle Cadenti fatte dai Membri dell' Associazione Meteorica Italiana durante l'anno 1871.—This volume forms Part II. No. VII. of the publications of the Royal Observatory of Milan, and consists of the observed paths of 9627 shooting-stars observed in Italy in 1871. This Catalogue is supplementary to two preceding ones, the first (published in 1874) contained 7152 meteors seen in 1872, and the second (published in 1882 and forming Part I. No. VII. of the publications as above) contained 7602 meteors. The three catalogues together include a total of 24381 observed in the years from 1868 to 1872 inclusive. The majority of these were seen by various observers at Montcalieri (6550 meteors) and Allessandria (2840 meteors), and Signor Maggi at Volpeglino individually contributed no less than 4389 of the meteors included in the three catalogues. With the exception of 4143 of the observations in 1872 * the whole of these meteors require reducing to their radiants. Anyone who will undertake this laborious work and bring it to a satisfactory termination will render an important service to meteoric astronomy. The catalogue before us contains several observations of those rare phenomena stationary meteors. We notice one seen by Maggi at Volpeglino on July 8, 1871, at the point 49° +58°, which seems to indicate that there is a shower of Perseids more than a month before the August display. This catalogue, in combination with the two previously published, will be of unquestionable value to those engaged in investigations on meteors, for not only are the apparent paths given, but many other useful data in regard to every meteor registered. The Italian Meteoric Association is to be commended for its perseverance in gathering such a mass of materials for future discussion. W. F. D.

Royal Alfred Observatory, Mauritius.—We have received the Report of this Observatory for the year 1886. The meteorological observations seem to have been conducted with the same zealous frequency as before. In comparing mortality tables with meteorological results, the Director remarks:—"We see that in the course of the last 16 years the total death-rate of the island has increased to the extent of 4·83 per 1000, and the fever death-rate to the extent of 5·95 per 1000. The amount of mortality no doubt depends partly upon the weather; but it does not appear that the increase in the death-rate is due to meteorological causes." The daily solar observations have been continued, and solar negatives and prints forwarded monthly to the Solar Physics Committee.

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* [The radiant-points of which were determined by Mr. Denning ('Monthly Notices,' vol. xxxviii. pp. 315-319).—Eds.]

Some of these are measured at Greenwich to fill up gaps in the Greenwich-Indian series due to bad weather.

Parallax of Nova Andromedæ.—A few measures of the distance of this star from neighbouring stars, made by Herr Franz, indicate a negative relative parallax (Ast. Nach. No. 2816).

The Nebulæ in the Pleiades.—We are glad to hear that MM. Henry have succeeded in taking a photograph of the Pleiades, with an exposure of four hours, which shows very much more nebulous matter than the well-known photograph they took last year. We hope to give more particulars of this, their latest achievement, next month.

We regret to announce the death of Professor Balfour Stewart, M.A., LL.D., F.R.S. Mr. Stewart, who had just completed his 59th year, was educated at the Universities of St. Andrews and Edinburgh; in 1859 he was appointed Director of the Kew Observatory, and in 1867 to the secretaryship of the Meteorological Committee; in 1870 he was appointed Professor of Natural Philosophy in Owen's College, Manchester. He was also President of the Physical Society of London. His death took place on Monday, Dec. 19, at Ballymagarvey, Balrath, in the county of Meath.

Ephemeris for Olbers' Comet, 1888 *.

For Berlin Midnight.

1888.	R.A. h m s	Decl. ° ′	Log *r*.	Log Δ.
Jan. 2..	16 42 30	−0 39·0	0·2389	0·3783
4..	16 46 27	1 1·2		
6..	16 50 20	1 22·7	0·2486	0·3821
8..	16 54 8	1 43·4		
10..	16 57 51	2 3·4	0·2583	0·3854
12..	17 1 29	2 23·6		
14..	17 5 3	2 41·2	0·2679	0·3884
16..	17 8 31	2 59·2		
18..	17 11 55	3 16·6	0·2773	0·3910
20..	17 15 14	3 33·4		
22..	17 18 29	3 49·6	0·2866	0·3932
24..	17 21 39	4 5·3		
26..	17 24 44	4 20·4	0·2958	0·3948
28..	17 27 44	4 35·0		
30..	17 30 40	4 49·2	0·3048	0·3961
Feb. 1..	17 33 31	5 3·0		

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* Astr. Nach. No. 2818.

Ephemeris (*continued*).

	R.A.			Decl.			
1888.	h	m	s	°	′	Log *r*.	Log Δ.
Feb. 3..	17	36	17	−5	16·3	0·3137	0·3969
5..	17	38	59	5	29·2		
7..	17	41	36	4	41·7	0·3224	0·3974
9..	17	44	8	5	53·9		
11..	17	46	35	6	5·7	0·3320	0·3974

From a single comparison with a star on Dec. 12 by Dr. E. Lamp the correction to the above ephemeris is +8^{s} in R.A.; the Declination is correct.

Minima of R Canis Majoris and Y Cygni.

The following ephemerides of the new Algol type Variables for 1888 are given by Mr. S. C. Chandler, Jun., in the Astr. Journal, No. 163.

R Canis Majoris.

Brightness at maximum 5·9; at minimum 6·7.

Duration of decrease 2½^{h}; of increase 2½^{h}: stationary maximum 22^{h}.

					h m
Jan. 1	Jan. 26	Feb. 20	Mar. 16	Apr. 10	5 20·1
2	27	21	17	11	8 36·0
3	28	22	18	12	11 51·9
4	29	23	19	13	15 7·8
5	30	24	20	14	18 23·7
6	31	25	21	15	21 39·6
8	Feb. 2	27	23	17	0 55·6
9	3	28	24	18	4 11·5
10	4	29	25	19	7 27·4
11	5	Mar. 1	26	20	10 43·3
12	6	2	27	21	13 59·2
13	7	3	28	22	17 15·1
14	8	4	29	23	20 31·0
15	9	5	30	24	23 47·0
17	11	7	Apr. 1	26	3 2·9
18	12	8	2	27	6 18·8
19	13	9	3	28	9 34·7
20	14	10	4	29	12 50·6
21	15	11	5	30	16 6·6
22	16	12	6	May 1	19 22·5
23	17	13	7	2	22 38·4
25	18	15	9	4	1 54·3
+ 9^{m}·8	0^{m}·0	− 9^{m}·8	− 19^{m}·6	− 29^{m}·2	

The time of minimum is found by applying to the time in the right hand column opposite the date the correction at the foot of the column.

Y CYGNI.

Brightness at maximum 7·1; at minimum 7·9.
Duration of decrease 4^h; of increase 4^h: stationary maximum 28^h.

	d	h	m		d	h	m		d	h	m		d	h	m
Jan.	1	9	25·7	Feb.	12	7	56·1	Mar.	25	6	26·5	May	6	4	56·9
	2	21	22·5		13	19	52·9		26	18	23·3		7	16	53·7
	4	9	19·3		15	7	49·7		28	6	20·1		9	4	50·5
	5	21	16·1		16	19	46·5		29	18	16·9		10	16	47·3
	7	9	12·9		18	7	43·3		31	6	13·7		12	4	44·1
	8	21	9·7		19	19	40·1	Apr.	1	18	10·5		13	16	40·9
	10	9	6·5		21	7	36·9		3	6	7·3		15	4	37·7
	11	21	3·3		22	19	33·7		4	18	4·1		16	16	34·5
	13	9	0·1		24	7	30·5		6	6	0·9		18	4	31·3
	14	20	56·9		25	19	27·3		7	17	57·7		19	16	28·1
	16	8	53·7		27	7	24·1		9	5	54·5		21	4	24·9
	17	20	50·5		28	19	20·9		10	17	51·3		22	16	21·7
	19	8	47·3	Mar.	1	7	17·7		12	5	48·1		24	4	18·5
	20	20	44·1		2	19	14·5		13	17	44·9		25	16	15·3
	22	8	40·9		4	7	11·3		15	5	41·7		27	4	12·1
	23	20	37·7		5	19	8·1		16	17	38·5		28	16	8·9
	25	8	34·5		7	7	4·9		18	5	35·3		30	4	5·7
	26	20	31·3		8	19	1·7		19	17	32·1		31	16	2·5
	28	8	28·1		10	6	58·5		21	5	28·9	June	2	3	59·3
	29	20	24·9		11	18	55·3		22	17	25·7		3	15	56·1
	31	8	21·7		13	6	52·1		24	5	22·5		5	3	52·9
Feb.	1	20	18·5		14	18	48·9		25	17	19·3		6	15	49·7
	3	8	15·3		16	6	45·7		27	5	16·1		8	3	46·5
	4	20	12·1		17	18	42·5		28	17	12·9		9	15	43·3
	6	8	8·9		19	6	39·3		30	5	9·7		11	3	40·1
	7	20	5·7		20	18	36·1	May	1	17	6·5		12	15	36·9
	9	8	2·5		22	6	32·9		3	5	3·3		14	3	33·7
	10	19	59·3		23	18	29·7		4	17	0·1				

ERRATA IN COMPANION FOR 1888.

P. 19. Eclipse V., last line but 6, *for* 157° 39′·6 E. *read* 137° 39′·6.—The longitude was taken from the American N.A., which appears to be 20° in error. The English N.A. is correct. In Eclipse IV., on the other hand, the English N.A. should be corrected in the angles for contacts with the shadow. The American N.A. and the 'Companion' are correct.

P. 37. Change the sign of P throughout; as given, the position-angle would be read in the direction N., W., S., E.

P. 49, *for* γ Cygni *read* Y Cygni.—A better ephemeris of this star is given in the present number of the 'Observatory.'

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 133. FEBRUARY. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, January 13, 1888.

J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretary : E. B. Knobel.

The Minutes of the previous Meeting were read and confirmed.

Mr. Knobel. 60 presents have been received since the last Meeting, including vol. ix. of the 'Observations of the Cordova Observatory in the Argentine Republic,' and a further series of beautiful photographs of the solar spectra made by Mr. Higgs.

A vote of thanks to the donors was passed, especially to the donors of the presents mentioned by Mr. Knobel.

Rev. Father Perry laid before the Meeting some notes on the solar surface, which he described as a short account of some more or less interesting facts which had been observed on the Sun at Stonyhurst in 1887. The paper was illustrated by drawings and by a curve showing the areas of the spots throughout 1886 and 1887. The number of days in each month upon which drawings were obtained were respectively 11, 18, 24, 22; 22, 28, 30, 25, 21, 23, 17, and 18, forming a total of 259 in the course of the year. He mentioned these numbers in order to show that a great deal of solar work could be done in England in spite of the adverse climate. The areas had been reduced to millionths of solar hemisphere. The first point that struck the eye on viewing the graphic representation of the areas was the prolongation of the minimum period during the first four months of the year, with its sudden cessation at the opening of May. There were already in this eleven years' period two minima, one lasting from September 22nd to December 8th of 1886 (or for two and a half months), and the other extending over the first third of 1887, more than twice as many days without spots being noted in the second period than in the first. But if both these periods were included in the same minimum, by neglecting the interruption at the close of 1886, the whole minimum period would include 222 days, and the date of the minimum may be given approximately as January 10th, 1887.

In thus speaking of the minima, he did not wish it to be inferred that the absolute minimum of the eleven-years' period was reached on January 10th, 1887; it would, of course, be premature to venture upon such an assertion, as even longer periods of solar inactivity might be looked forward to during the years 1888–89. In the course of the year that had just expired, viz. from May to August, there was a considerable wave of disturbance corresponding to each rotation of the Sun, and these differed but little in extent, although they were by no means periodic returns of the same groups to the visible hemisphere. Most of the larger spots and groups had this year lasted but for a short time, one group only (viz. that first observed on January 27th) having lasted during part of three revolutions. This group was finally observed as three small dots on March 27th. On seven days only was the Sun observed to be free from spots between May 21 and August 24, but from this date to November 30 the solar surface was more quiet, and thirty-five days were recorded as spotless. During the last month the spot-area had been larger than in any month since May 1886, December 15th being the date of the secondary maximum. The group visible from July 22nd to 30th was a typical case of the formation of a spot, a slight dot surrounded by a few faculæ being the first indication of a coming disturbance. This instance might throw some light on the important question whether faculæ preceded spots or spots faculæ. He would like for a moment to call attention to the secondary maximum which occurred in 1886. The maxima of the spot-curve occur in the following order as regards magnitude, viz.:—March 8th, May 2nd, and April 2nd, the spot-area (in millionths of the hemisphere) being respectively 2459, 2274, and 1575, which agrees much more closely with the results published by Dr. Wolf of Zurich than with epoch given by Prof. Tacchini.

Another point of some interest was the conduct of the veiled spots during the period of minimum. These spots had been divided into three classes. The first comprised misty patches, large but of short duration; these had scarcely been seen at all this year, and there seemed to be a great difference between maxima and minima with regard to them. The second class consisted of penumbral dots which increased in size and then coalesced with other spots in their immediate neighbourhood. This class of spots had been quite as striking and as universal as during the maximum period; but the individual groups were somewhat less extended in 1887. The third class of veiled spots, which differed from the others in their duration, lasting often for several days, and also in their close resemblance to minute patches of penumbra, had been observed this year rather less than in 1883 and 1884. These faint patches had been examined both on the projected image and directly by aid of the solar prism and a Dawes's eyepiece; and it was invariably found that the observation was far more easy and perfect when the Sun was viewed by projection.

On October 28th at $9^h\ 8^m$ a few faculæ attached to a group of faint spots became of a sudden intensely bright and faded again as quickly. During this appearance no other change of any importance occurred in their neighbourhood. Within three minutes both the faculæ and the spots had entirely disappeared.

Capt. Noble. There is one point of interest in Father Perry's paper to which I should like to refer. There is a school of persons in this country who are, happily, more noisy than numerous, who insist upon the connection of Sun-spots with all sorts of things—famines, cyclones, and commercial panics. Now it would be a very curious thing indeed to compare the statistics of these phenomena. I do not know whether you could call commercial panics phenomena; but, at any rate, compare cyclones with these peculiar maxima, because all the alleged correspondences go upon the assumption that there is a well-marked period of 11·1 years; and, if we are going to have subsidiary maxima and the maxima and minima are going to be as irregular as these observations seem to show they are, it seems to me that the alleged correspondent phenomena ought to be irregular too; and my point is that some comparison should be made with real observations, like Father Perry's, not to serve any end, but simply to see what is going on in the Sun. They should not be "cooked" and the curves smoothed, though that is the adopted way of doing the thing. You can get a smoothed curve to fit anything; but we should have really legitimate observations like Father Perry's compared with these alleged phenomena, and see how far they correspond.

Rev. T. Harley. One point of some interest to me is the number of days during which solar observations were taken last year. Something, perhaps, might be made of that. After the brilliant week we have just had it is refreshing to know that the Sun was seen last year during 259 days; I think nearly 70 per cent. is rather above the average. It would be interesting to take a period of, say, twenty years, and ascertain during how many days *per annum* we could take these observations in England; and then see during how many days they could be taken in Paris, Berlin, Rome, and Washington. In the United States, where I lived for several years, I was continually asked whether we had fog in London all the year round. It is gratifying to know that we can see the Sun here so often, and I think it would be not only interesting, but perhaps useful, if we could obtain an induction with regard to this matter as to how many days in the year, on the average, we might take solar observations. We have also many beautiful nights in this country, which some people outside England do not know: they imagine we are involved in gloom through all the year. I would like to hear something more on this point from those who have made observations extending over a number of years.

Mr. Stone. I think Captain Noble has been rather severe on those gentlemen who try to trace a connection between sun-spots and terrestrial phenomena. I must confess that some years ago, when I made a comparison between the Cape meteorological observations and Wolff's changes of the number of sun-spots, I was very much struck by the fact of there being close agreement of the law of changes, not only for the principal maxima but also for the subsidiary maxima. The mistake which has been made with regard to these periodical phenomena is, I think, something of this kind:—We do not know the law of the changes going on in the heating-power of the Sun, and we do not know whether the greatest heating-power corresponds with the time of minimum of sun-spots or not; but I think we may be quite certain of this, that if the changes in the distribution of sun-spots are due to changes of the heating-power of the Sun, there will be corresponding changes with the same period, though not necessarily the same epoch, running through all our meteorological elements due to the changed effects of the Sun's heating-power; and, although you must not expect to get at all stations a maximum temperature or a great rainfall corresponding exactly to a maximum or a minimum of sun-spots, yet there will be periodical changes with the same period as that of the sun-spots. The epoch of greatest effects at each station would have to be found from observation. So far as my observations went, the effects appeared to be very small, but there appeared to be a real connection between the periodicity of the spots and the meteorological changes which had taken place at the Cape station, and probably similar results would follow comparisons at other selected stations, but I do not think that we can infer that all the Earth over there would be a greater rainfall or less rainfall at a period of a minimum of sun-spots or conversely.

Mr. F. M‘Clean asked if Father Perry had made any observations on the comparative permanency of the sun-spots and the solar prominences. He added that, for nine years past, he had himself made a certain number of drawings of the Sun showing both spots and prominences, and that, so far as they went, by comparing the recurrence of the prominences as the same longitude came round successively to the limb of the Sun, there seemed to him indications that prominences were of a more permanent character than sun-spots. This would appear to agree with the supposition which had been put forward that the spots were formed of the gaseous products of combustion from the prominences, as they made their way down again into the body of the Sun.

Rev. Father Perry. The prominences are measured daily as well as the spots; but the observations at Stonyhurst would be quite insufficient to establish a close connection between the two. The decrease, however, is very clearly marked in 1887 in the number, height, and extent of the prominences, and so far it agrees with the minimum spot area.

Mr. Stone laid before the Meeting a paper on some observations of the Moon made in 1887, and a comparison of the results with the tabular places from Hansen's Lunar Tables.

Mr. Stone observed that he did not think there was very much in this paper to be read. Hansen's Tables had been carefully compared with observations of the Moon for many years. For some reason or other there had been a great discordance between the theoretical expression and the observed longitudes since the year 1864. The illustrative curve showed clearly what had been going on. The line of mean vibration of outstanding periodical effects had completely changed about 1864, and the difference between the tabular places and the observed places, which was about $-1''\cdot5$ for some years before 1863, had since progressively increased, and was now nearly $+16''$, so that it had changed about $17''$ in the last twenty-four years. He thought it extremely desirable, if any advance is to be made in gravitational astronomy, to keep clearly under view the law of the increasing discordance between observation and the theoretical expression, which was nothing more than a definite assigned algebraic expression, including a lot of periodical terms which could not alter *per saltum* its mean rate of change per unit of time, until some physical explanation satisfactory to astronomers had been brought forward. His own explanation was in accord with the facts; and he considered that he had strictly proved its truth independently of the evidence afforded by the Lunar observations. But if his explanation was rejected, some other must be supplied. To merely throw away the discordances between theory and observations by fresh empirical alterations of the long inequalities every few years, directly a discordance was put clearly in evidence, was absolutely fatal, in his opinion, to the advance of the science. We might just as well have disguised the law of errors in the Tables of Uranus by fresh determinations of the elliptic elements every few years; and had this been done, the discovery of Neptune would have been lost to theory.

Mr. Bryant laid before the meeting a paper on "The Opposition of Sappho, containing an Ephemeris for the Opposition next March and April," with a list of comparison stars, of which he would ask observers to make use.

In August and September next year the planet would make one of those near approaches to the Earth which, on account of the eccentricity of its orbit and the near commensurability of its period with that of the Earth, occurred every 7 years. He had made a rough calculation, from elements osculating at some time during this spring, that the near approach of the planet to the Earth would take place at the beginning of October next year. It was only a four-figure calculation, so that high accuracy was not to be expected, nor was it requisite. It appears that the parallax of the planet would amount to about 10 secs. of arc, and a most favourable opportunity would then be offered for determining the solar parallax. He thought that there was great need for such a determination. If they could get solar parallaxes within one or two hundredths of a sec. as had lately been done, why not employ the opposition next year for the determination of that quantity, with the importance of which they were all acquainted? Mr. Common had told him that the planet could be photographed in 10 seconds. He believed that Dr. Gill did not intend to observe the planet next year with the heliometer, but to devote his attention to Victoria, which would be rather brighter.

Mr. Turner. Can Mr. Bryant give us offhand the motion of the planet in 10 secs. of time?

Mr. Bryant. I have not yet calculated the ephemeris for next year, but on account of the mean motion being so nearly commensurate with the Earth's, the ephemeris will not differ very much from 1882. I find the motion in 1882 amounted to 600 seconds of arc in R.A. in a day. The motion in declination was greater, namely 800 seconds of arc. This would be about half a second in a minute. That would be a very small quantity, but by bisecting the image on the photographic plate the middle point of the streak will be taken to correspond to the mean of the time of exposure, so that any small error due to the planet's motion would be almost eliminated.

The Astronomer Royal, in introducing his paper on the "Spectroscopic determinations of the Motions of Stars in the line of sight, made at Greenwich in 1887," said there were just two points which he thought might be of interest. One had reference to the motion of Sirius. As Fellows of the Society were aware, this star had shown a complete reversal of its motion during the time of observation at Greenwich, and since Dr. Huggins's first results. Having tabulated the results he thought this might be a convenient opportunity to communicate them. In 1868 Dr. Huggins found the motion to be +29 miles a second, receding from the Earth, and in 1872 +18 to 22 miles a second. In the winter of 1875–76 the motion, as found from the Greenwich observations, was +24 miles a second. Subsequent years showed the following results:— 1876–7, +12; 1877–8, +23; 1879–80, +15; 1880–1, +11; 1881–2, +2; 1882–3, −5; 1883–4, −19; 1884–5, −23; 1885–6, −24; 1886–7, −1; and for the year 1887, +6; last year, therefore, turning to a small plus again. These results must, of course, be accepted with some caution, as regards the conclusion that the apparent change in the displacement of the F line indicated a real change of motion in the line of sight. The line observed in these observations was always the F line, a very broad line in the spectrum of Sirius, and that line had somewhat changed in its character, so that it might be that there had been some change of the absorption of the atmosphere of Sirius which might have affected the line without really indicating a change in the motion. That was one difficulty in interpreting these results: they were not quite certain that these quantities represented the real displacement of the F line in Sirius as compared with the hydrogen line obtained through a vacuum-tube. It was possible that mixed up with this F line in Sirius there might be some other line which interfered with the observation. Then, again, there came the question of whether there might not be changes in the motion at different times of the year, because in the spectroscopic method the motions which were measured were, of course, actual velocities, and a rapid motion in a very small orbit might be discovered with the spectroscope which would be quite insensible in a telescope, and such a motion might have a period of a few months. He merely gave these results without venturing to express any opinion as to the cause of the change.

In 1879 Sirius was not observed. In the earlier years so many observations were not obtained as in the later ones. In the opposition of 1875–6 there were 6 observations; in 1876–7, 2 on a single night; in 1877–8, 8; in 1879–80, 10; in 1880–1, 4; in 1881–2, 22; in 1882–3, 18; in 1883–4, 43; in 1884–5, 28; in 1885–6, 28; in 1886–7, 24; and during the present opposition 6 observations on one night, so that there are a great many more measures in these later years.

Mr. Ranyard. How do they agree with one another for each year?

The Astronomer Royal. I really cannot say offhand. Perhaps the best way will be to read out the results for last year. They differ considerably *inter se* in this year; the first result was −26, the second +13, the third −12, the fourth −7, the fifth +35.

Mr. Ranyard. That shows nearly as great a range as from year to year.

The Astronomer Royal. In the individual observations, yes: but when you come to take the mean of a large number, of course the result deserves greater confidence. At the same time, I must point out that the star-line in Sirius is extremely difficult to bisect, and that it has lately changed in its character, whilst the whole of the range in these values represents really a small fraction of the breadth of the line, which corresponds, perhaps, to about one thousand miles a second. The great difficulty in these measures is in the diffused character of the object we have to measure, so that if the line changed its character somewhat, and we were to get a condensation more pronounced than before, that might be taken on very fine nights, while the middle of the line would, perhaps, be taken when the definition was not so good.

Capt. Noble. As long as the line remains constant, the observations are simply differential.

The Astronomer Royal. Yes: the point appears to be that the line has changed somewhat, and it is quite possible there may have been some change in the estimation of its centre at different times.

Mr. Bryant. If Sirius possesses a companion we should expect such a discordance as is there represented, where the very small positive quantity of $4\frac{1}{2}$ is the general mean throughout. It seems difficult, however, to represent them by simple formulæ.

The Astronomer Royal. The numbers might represent orbital motion, of course?

Mr. Bryant. Orbital motion, as well as motion radially from the Sun, represented by the general mean.

The Astronomer Royal. I do not think these results would fit in with the recognized orbital motion of Sirius upon any plausible assumption.

Mr. Bryant. I mean to put the two together, the motion of the orbit and the motion from the Sun.

The Astronomer Royal. This change of motion is very much larger than any that would appear probable from the known motion of Sirius in its orbit.

Col. Burton Brown. I should like to ask if the observations in 1880–1881 were within a short period of each other, or extended over the whole year.

The Astronomer Royal. I am afraid I cannot say at this moment*. All the observations are published; my object in bringing them before the Society is to draw attention to them; anyone can examine the results. They are given year after year. I may say I am not bringing this matter forward as a complete investigation, because there are great difficulties in interpreting the results up to the present time.

Another point which may be of interest is with regard to the orbital motion of Algol. The hypothesis started to explain the variability of that star is that there is an opaque body or cluster of bodies which circulates round Algol, and causes this rapid diminution of its brightness. If that were so, one would expect that Algol itself would have an orbital motion which might possibly be measurable with the spectroscope. Results have been obtained in different portions of the supposed orbit, but they are still incomplete, as I do not think we have enough observations to determine so delicate a point; and the observations are subject to very large errors.

I may say here that there is a very great difference in the character of the lines in different stars, and a striking point that comes out in comparing the observations which have been made at Greenwich for a great number of years is, that whilst we get quite consistent results with certain stars, such as Vega, Arcturus, &c., with others we can get no trustworthy results. A great deal depends upon the character of the line. A curious circumstance with reference to Altair is, that when we first began to observe it we found we got most discordant results, and after a time we gave up the attempt as hopeless, as it seemed that the probable error of observation so largely exceeded the quantity we had to measure, that no useful result was likely to be obtained. But now we get fairly good results. It seems that the character of the line has changed, and that it can be measured with a fair degree of accuracy.

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* The observations in 1880 were made on Jan. 30, March 19 and 24, and in 1881 on Feb. 16 and 28, and Dec. 7.

Algol appears to be a star for which there is a large probable error of observation with the spectroscope. I do not think there is conclusive evidence of orbital motion here, but there is a suggestion of it, and the discordances are quite consistent with the probable errors one might expect. The crucial test is this: if the star is going round in an orbit so as to pass behind the opaque body at the minimum of brightness, a quarter of the period after the minimum it must be approaching us, while on the other side it must be receding, as regards the orbital motion. The point that comes out is, that as a matter of fact, according to these observations, it is approaching us at the first point more than at the other. We may take it that there is a residual motion of approach in the whole system, and you see that on one side it is approaching at the rate of 59 miles per second, whilst on the other it is approaching at the rate of only 23, so that there is a difference between the two representing the difference of orbital motions, and what strikes one is, that that is in the right direction. If it had been the other way, it would not have fitted in with the hypothesis. At the same time, the measures made on other parts of the orbit, near minimum and opposite, give a rate of approach of about fourteen miles per second, which is decidedly less than the mean of the other two. However, the discordances are not greater than would be covered by the probable errors of observation. In mentioning this, of course I am understood to guard myself by saying that further observations are required to establish anything definite from these results.

Mr. Ranyard. I should like to say with regard to the very important evidence the Astronomer Royal has brought forward with respect to the shift of the F line in the spectrum of Sirius, that it does not seem to me probable that the shift can be explained by a slow change in the physical condition of the star, assuming this F line to correspond with the Hydrogen line. A slow change of discordance with the Hydrogen line would be most simply and easily accounted for by actual motion of the star in the line of sight, without assuming the existence of a new and very remarkable physical fact; to my mind it seems that the most simple explanation is the one which the Astronomer Royal first suggested.

The Astronomer Royal. With regard to that point, it is not necessary to assume that there is a shifting of the F line, but there may be other absorption lines in the atmosphere of Sirius which have got mixed up with the F line, and which may cause confusion between the point which is taken as that for observation and the real centre of the F line. One thing which has struck me very much in the case of the Sun has been the appearance of very strong absorption bands between *b* and F, seen in the sun-spots first by Mr. Maunder at Greenwich in November 1880. These bands did not correspond to any line in Angström's map, and we could not make out any coincidence with any known line or band. After some years these bands resolved themselves into a number of fine lines, and changed their character completely, and then gradually faded out. During one or two years, we saw these bands in most spots, and after some years they resolved themselves into fine lines, indicating diminished absorption, and afterwards they did not show at all. If we have such changes going on in sun-spots, they are far more likely in such a hot star as Sirius. We must not conclude, because we get an apparent displacement, that that indicates real motion in all cases.

Mr. Maxwell Hall. I think that the observations of which the Astronomer Royal has given us a notice this evening, are particularly interesting, especially those which refer to Algol, for this reason: supposing he observed the velocity of the rotation of the bright body round the common centre of gravity of the two bodies to be v miles per second, by multiplying v by the period expressed in seconds we clearly get the *circumference* of the orbit described by the bright body round the centre of gravity expressed in miles. We also get the *distance* in miles of the bright body from the common centre of gravity; and if we go on further, we have the ratio of the radii of the dark and bright bodies, and we have also the ratio of the radii of the two bodies to the common distance between them; and so the whole theory comes out complete, always upon the assumption that the extinction of light is due to a dark body revolving round a bright one. I may here mention that if we take Schönfeld's observations of the variation of the light, we have 20·8 and 5·6 as the numbers which express the light at maximum and minimum respectively; whence we get 0·86 as the ratio of the radii, that is to say, the radius of the smaller dark body is to the radius of the bright body as 0·86 is to 1. Again, we find that the ratio of the radii, which do not greatly differ, to the common distance between the two bodies is 0·22, or as 1 is to 5. And further, if we make use of the algebraic equation which embodies Kepler's Third Law of Motion, we can actually deduce the ratio of the density of the component bodies to the density of the Sun. The density comes out remarkably small. From the numbers just given we shall find that the density is only about one-tenth that of water. This is much smaller than might have been expected; and when we take into consideration that the dark body really eclipses the light very suddenly, there is reason to suppose that the dark body is composed of meteorites, as suggested by other writers besides myself. Whether this be possible or not I do not really know; still it seems very strange that the density of the bodies should be so remarkably small. Finally, if we were to assume that the Astronomer Royal will eventually find the velocity of rotation round the common centre of gravity to be 12 miles per second, we shall ultimately find that the diameter of the two bodies is about one fourth that of the Sun, and that the distance between them is about one and a half million miles. I only give these figures to show how interesting the subject is; and I hope that my remarks may lead to further research, and to the explanation of the cause of variation of light in this particular class of variable stars.

Mr. Knobel read a paper by Mr. Hind entitled "A Note on the Total Eclipse of 1889."

Mr. Turner. Although this eclipse is situated in a very interesting locality it is rather of short duration. The only two eclipses likely to be observed for the next two or three years are this one and one in December of the same year, which is only visible at one accessible place, on the west coast of South Africa, about 50 miles south of St. Paul de Loanda. I have been making enquiries at St. Paul de Loanda whether the conditions will be favourable, and I hear from the Consul there that the meteorological conditions will be very favourable, but it is rather a difficult place to get at. In fact the spot where the central line strikes the coast is a place he has never heard of. Totality is of rather long duration, about 3½ minutes, and I think that with a little enterprise it may be found a very favourable opportunity for observing a solar eclipse.

Mr. Turner then referred to a paper by the *Astronomer Royal* on "Observations of the Occultations of the Stars by the Moon and Phenomena of Jupiter's Satellites at the Royal Observatory, Greenwich." He said that they had had a remarkably successful year in the way of occultations at Greenwich. During the last 20 years he found that the number of occultations of stars given in the 'Nautical Almanac,' as visible at Greenwich, was about 1600. The percentage observed at Greenwich for these 20 years was something like 10 per cent. for disappearance and 5 per cent. for reappearances. In 1887 the percentages were nearly double these. That was due to two circumstances. In the first place, there was every few years a larger number of occultations of stars, and this was one of the favourable years; and, secondly, the occultations in those years were apt to be of larger stars, and therefore more interesting. They had had several opportunities of looking for so-called "projection" on the Moon's limb; notably at two occultations of Aldebaran and one of Regulus. In those cases the only notes which were made on the subject were that some roughnesses of the Moon's limb seemed to have interfered with the observations, and the phenomena seemed to have been complicated thereby; in one case the star seemed to reappear in two portions, and in another case to slide down a roughness on the limb.

Mr. Common laid before the meeting a paper upon a method of testing polished flat surfaces, observing that it was very short, but might be of special interest to Fellows using reflecting telescopes.

General Tennant then read a "Note on the Definition of Reflecting Telescopes, and on images of bright stars on Photographic Plates." He said that although some English astronomers had never seen rings he was very much struck by the fact that Foucault always saw rings. MM. Henry had informed him that not only did they always see rings, but the rings were better seen with a reflector when properly finished than with a refractor. He referred to the diffraction-rings round the image of a star. M. Henry distinctly said that when people did not see the rings it must be because there were outstanding aberrations. His observations agreed entirely with the theory that the ring was conspicuous and easy to see. It was very strange that no English observer had noticed these rings round the stars. He had made some calculations, the result of which was that with a central disk one third the diameter of the Great Speculum the appearance of a star would be modified; there would be a smaller disk, and the first ring would be wider and brighter, indeed three times as bright as with the whole speculum. He found that when a star of the 14th magnitude was impressed on a photograph the light so visibly marked was $\frac{1}{149000}$ that of a star of the 1st magnitude, and that the image must extend far beyond all the rings, and be so faint that it must have been impossible for the optician to secure its being concentrated in the focus, and this might partly explain the great size the impression reached.

The following papers were announced:—

W. F. Denning. "The Chief Meteor Showers."

W. F. Denning. "Heights of Fireballs and Shooting-Stars."

T. W. Backhouse. "Nebula in Andromeda and Nova 1885."

Rev. S. J. Perry. "Notes on the Solar Surface of 1887."

John Tebbutt. "Observations of the Phenomena of Jupiter's Satellites made at Windsor, New South Wales in the year 1887."

A. Marth. "Ephemeris of the Satellites of Mars 1888."

Lt.-Col. G. L. Tupman. "On the Cross Reticule."

R. Bryant. "The Opposition of Sappho in 1888."

E. J. Stone. "Observations of the Moon made at the Radcliffe Observatory, Oxford, during the year 1887, and a Comparison of the results with the tabular places from Hansen's Lunar Tables."

Lt.-Gen. J. F. Tennant. "Note on the Definition of Reflecting Telescopes, and on the images of Bright Stars on Photographic Plates."

Communicated by the Astronomer Royal. "Spectroscopic Results for the Motions of Stars in the line of sight made at the Royal Observatory, Greenwich, in the year 1887.—No. XI."

Communicated by the Astronomer Royal. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, during the year 1887."

A. A. Common. "Note on Testing Polished Flat Surfaces."

J. R. Hind. "Note on the Total Solar Eclipse of 1889, Jan. 1."

Edw. E. Barnard, M.A., Lick Observatory, Mount Hamilton, California; *William R. Brooks*, Red House Observatory, Phelps, New York; *Capt. Leonard Darwin, R.E.*, 18 Wetherby Place, South Kensington, S.W.; *James Gill*, 2 Beech Mount, Beech

Street, Liverpool; *William R. Hutchinson, C.E.*, Liversidge; *George James Jacobs*, Spital Street, Guildford, Surrey; *Rev. Joseph Walshe*, St. Edward's College, Liverpool; *Francis James Wardale*, 1 Whitehall Place, S.W.; were duly elected Fellows of the Society.

Arthur Berry, B.A., King's College, Cambridge; *The Rev. Walter Edward Stewart, M.A.*, 1 Bank's Terrace, Croft-on-Tees, Darlington; were proposed for election as Fellows of the Society.

The meeting adjourned at 10^{h} 0^{m}.

Liverpool Astronomical Society.

The fourth meeting of the Session was held at the Royal Institution on the 9th inst., Mr. James Gill, F.R.A.S., Vice-President, in the chair. Thirty new members were elected and proposed, and the following were elected Associates of the Society:—Miss Agnes M. Clerke, Dr. G. F. J. Arthur Auwers, Mr. A. A. Common, F.R.S., Dr. J. W. L. Glaisher, F.R.S., and Admiral E. Mouchez.

In a note on the "Rotation of Jupiter," Mr. Denning remarked that Mr. Lynn's belief appeared to be that because Mädler and Airy had found the rotation-period in 1835 to be 9^{h} 55^{m} 27^{s}, and that the period of the red spot is now 9^{h} 55^{m} 41^{s}, therefore the diurnal rate was increasing to the extent of $0^{s}{\cdot}27$ of time annually. But Cassini noticed spots which gave periods of 9^{h} 56^{m} and 9^{h} 55^{m} 50^{s} some 200 years ago; and in 1692 an equatoreal spot was seen rotating in 9^{h} 50^{m}. Mr. Denning remarked that his results were amply confirmed by Prof. Hough and other leading observers. Mr. Sadler observed that he was unable to verify the mean period of Mädler and Airy as given by Mr. Lynn.

Reporting on "Sun-spots," Miss Brown said that, in the absence of any occurrence of noteworthy interest in solar observations, it might be worth while to record two groups of spots which appeared in December last. The low altitude of the Sun and unfavourable weather rendered it difficult to obtain any connected record of their progress; but a few drawings were made by two observers which seemed to show that the groups exhibited rapid changes, and were accompanied by a great extent of penumbra.

In a paper on "Mercury," Mr. Denning stated that it was scarcely to be wondered at that our knowledge of the appearance and physical conditions of this planet was so meagre, inasmuch as it was so rarely visible to the naked eye, and was seldom to be observed under satisfactory conditions. Its diminutive size, moreover, was a hindrance which could not be overcome; but those who were able to select suitable times for their observations might, especially if provided with equatoreal telescopes, obtain views of Mercury showing his outlines with extreme sharpness. In November 1882 he obtained more reliable views than he had been able to secure either before or since. On several occasions in that month he was able to observe light and dark markings so pronounced as to suggest an analogy to those of Mars rather than to the faint and delicate details visible on Venus. On November 25 he estimated the planet to be brighter than Sirius. Mr. Denning alluded at length to some very interesting observations by Prof. Schiaparelli at Milan, which tended to confirm his at Bristol.

The usual educational class was held after the ordinary meeting, under the direction of Mr. James Gill.

Royal Meteorological Society.

The Monthly Meeting was held on Wednesday evening, Jan. 18, at the Institution of Civil Engineers, 25 Great George Street, Westminster; Mr. W. Ellis, F.R.A.S., President, in the chair.

Messrs. C. H. Blackley, M.D., H. E. Brameld, E. S. Bruce, W. Forrest, J. Groves, M.D., C. Harrison, M.D., G. C. Harrison, H. R. Haswell, R. Laing, H. J. Lloyd, H. T. H. Mead, C. Perks, W. R. Pike, J. L. Rushton, M.D., and J. H. Walker were elected Fellows of the Society.

In a paper on "The Non-Instrumental Meteorology of England, Wales, and Ireland," Mr. G. M. Whipple, B.Sc., F.R.A.S., F.R. Met. Soc., discussed the observations of wind, cloud, thunderstorms, hail, snow, &c., made at the stations of the Royal Meteorological Society during the eight years 1878–1885, and published in the 'Meteorological Record.' The S.W. wind is the most prevalent, and blows, on the average, 74 days in the year; the W. wind occurs almost as frequently, blowing 65 days. The least dominant winds are the S.E. and N., which occur on 27 days, and the N.E. on 32 days. Thunderstorms are most frequent in the eastern and midland counties, and least in the north of Wales.

Subsequently the Annual General Meeting was held. The Report of the Council showed the Society to be in a satisfactory condition, the number of Fellows being 522.

Mr. Ellis, in his Presidential Address, reviewed briefly the work and position of the Society, remarking that such a Society, whilst unable to carry out expensive original or experimental work, could yet act with great advantage in inciting volunteer workers throughout the country to united action, of which one recent example was the ready response to the request of the Society for photographs of Lightning, an excellent collection, which would shortly be exhibited, having been obtained. Arrangements were also being made for the more systematic observation of thunderstorms. Referring to the question of sympathetic relation between sun-spots and magnetism and meteorology, he thought that any complete treatment of the question in its meteorological aspect seemed to require that it should be dealt with in a much more comprehensive manner than hitherto, for which purpose observations more completely covering the surface of the globe might be necessary, if indeed not necessary also for the solution of many other meteorological questions, the present meteorological stations being distributed over the Earth in such isolated clusters. The attention given to synoptic charts was most important, but the general meteorological characteristics of places should also still continue to be studied. After remarking upon other matters, he laid before the Meeting tables showing the monthly means of amount of cloud, from observations made in three different series at the Royal Observatory, Greenwich, extending in all from 1818 to the present time. In concluding, Mr. Ellis said that at one time the science of Meteorology seemed likely to form an exception to the general rule of advance, for more than any other it required the united action of many workers; but the field of inquiry opened out of late years allowed meteorologists to talk already of the New, or Modern, Meteorology, phrases typical of the advance achieved, although the knowledge gained seemed only to remind them of how much has yet to be done.

The following gentlemen were elected Officers and Council for the ensuing year:—

President.

William Marcet, M.D., F.R.S., F.C.S.

Vice-Presidents.

Francis Campbell Bayard, LL.M.
William Ellis, F.R.A.S.
Charles Harding.
Richard Inwards, F.R.A.S.

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Edward Mawley, F.R.H.S.
Charles Theodore Williams, M.A., M.D., F.R.C.P.

Meteor Notes.

THE following five meteors were recorded by Mr. D. Booth at Leeds during recent observations :—

Date.	h m	Mag.	From		To		Appearance.
			°	°	°	°	
Dec. 13...	11 10	♃	25½	+39	360	+25	Streak.
13...	11 22	♀	25½	+39	340	+24	Streak.
13...	11 22	2×♀	356½	+12½	354½	+12½	Very slow.
18...	10 56	♀	149½	+38½	143	+41½	Slow.
Jan. 2...	10 58	♃	40	+30	47	+ 4	Rather swift.

The meteor of Dec. 13, 11^h 22^m, was stationary for the first 2 seconds; then it moved very slowly a slight distance westward, throwing off a fragment about equal to a 1st mag. star, and there was a duple train apparently formed by sparks evolved from the upper and lower sides of the nucleus. The meteor was much foreshortened close to a radiant at about 5° +12°.

The meteor equalling Jupiter, seen at 10^h 58^m on Jan. 2, was noted at Bristol by the writer, who recorded the path as from 261° +61½° to 278° +64°. The motion was very slow. A comparison of the two observations shows that the radiant was in Draco at about 250° +57°, so that this meteor, though appearing on the night of maximum of the Quadrantids, did not belong to that stream, the radiant of which is placed some 13° to the W.S.W. As seen at Bristol the meteor seems to have been noticed much earlier in its flight than at Leeds, but a mean of the two observations places the initial height at 98 miles above a point west of Appleby, and the end height at 60 miles above Chester. The real length of path was 109 miles; the meteor was directed from N. 8½° E., and its inclination was 20⅓° to the mean horizon of the two places. The radiant-point is very near a shower seen at Bristol between Jan. 14 and 19 at 253° +56°. It may also be worthy of note that the fireball of Feb. 21, 1865, had a radiant 255° +55. The meteor of Jan. 2 last was also observed by Mr. Backhouse at Sunderland, who described it as equal to Sirius. It appeared near Orion, but its position was not particularly recorded.

Several meteors which have been registered at Bristol, and which were evidently near their radiants, and therefore directed nearly in a line towards the observer, have also been seen by Mr. D. Booth at Leeds, and in each case the Bristol observation indicates the height as considerably greater than that derived from the observations at Leeds. If the figures are perfectly reliable it would seem that the fore part of a meteor brightens up to a visible degree of incandescence much higher in the air than does the rear portion. It appears quite natural to suppose that the front surface of falling meteors is the part first ignited by the effects of atmospheric friction.

The shower of Quadrantids.—On Jan. 2 observations were obtained at Sunderland, Leeds, and Bristol, but moonlight prevented the smaller meteors being seen. At Sunderland Mr. Backhouse remarked that the moonlight had less effect than was expected, owing to the great transparency of the atmosphere. The maximum occurred rather later than usual, but observations were not possible before 10^h on Jan. 2 owing to clouds and rain. Mr. Backhouse has summarized his results in the following table. Column 1 gives the time of observations; 2, the number of minutes during which a watch was sustained (corrected for cloud, which was almost *nil*); 3, the hindrances to observation other than cloud; 4, the magnitude of the faintest stars visible; 5, the number of meteors observed; and 6, the rate per hour:—

1.	2.	3.	4.	5.	6.
h m h m					
10 3 to 10 49	32	Moonlight.	5½	4	7·5
10 51 to 11 46	41	„	5	8	11·7
11 46 to 13 37	69	„	5–5½	21	18·3
18 5 to 19 5	52	„	5–5½	13	15·0
19 5 to 19 15	10	Moonlight and twilight.	4½–4¾	3	18·0
19 5 to 19 21	6		8 –4	0	0·0
	210			49	14

Of the 49 meteors seen, 1 was = ♀, 1 = ♃, 6 = Sirius, 6 = 1st mag. stars, 15 = 2nd mag., 15 = 3rd mag., 4 = 4th mag., and 1 = 5th mag. Mr. Backhouse noted six Quadrantids in the 1½ minute preceding $13^h\ 6\frac{3}{4}^m$.

At Leeds Mr. Booth observed several meteors between 11^h and 13^h on Jan. 2. These included the fine meteor = ♃, previously referred to as observed also at Bristol and Sunderland, and a 1st mag. meteor moving quickly from 78° +32½° to 70° +35½° and obviously not a Quadrantid.

At Bristol between $10^h\ 43^m$ and 12^h on Jan. 2, six meteors were recorded: five of these were Quadrantids showing the radiant at 230° +49°. These meteors had long paths and moved with moderate speed.

Bristol, 1888, Jan. 16. W. F. Denning.

Selenographical Notes.

Marius.—Existing representations and descriptions of this interesting ring-plain, from the time of Schroeter to the present, display sufficient disagreement as regards the visibility, character, and position of certain features on the wall and in the interior to render it an object worthy of careful observation. The oldest drawing, deserving the name, is by the astronomer of Lilienthal and was made with his 7-feet reflector under morning illumination, 2 days 15 hours before full Moon, when the west wall of Reiner was on the terminator. In this he shows a little crater on the south-west wall, which was seen also by Schwabe in 1827. Both Mädler and, long subsequently, Neison failed, however, to detect it, though at sunrise and for some hours afterwards it may now be described as very conspicuous. Schmidt draws this object, and another of a similar type adjoining it on the east, prominently, together with a minute craterlet on the extreme southern point of the border (which at sunrise here exhibits a manifest gap). The more westerly of the two larger craters has frequently been seen by me as a bright crater full of shadow when Marius is just within the morning terminator, while at a somewhat later stage of sunrise I find it more or less dusky and difficult. At 9^h on December 26 1887, when it was observed under especially favourable conditions, it appeared as a deep oval crater, slightly unsymmetrical, being more elongated in a north-westerly than in an opposite direction, with a very brilliant eastern border. On the east of it, and nearly touching it, the other object recorded by Schmidt was obvious—resembling a deep irregularly-shaped cavity or depression with no definite boundary, except on the east, where a portion of its inner slope seemed to be illuminated by the rising Sun. Both these craters were also seen by Mr. G. T. Davis of Theale with a 3¾-in. Wray refractor on 6th January 1886, when the morning terminator lay 18° or more east of Marius and nearly three-fourths of the area of the floor was illuminated. He saw in addition a large circular pit on the outer slope of the eastern wall, which, I believe, has not been previously recorded. Mr. W. M. Maw of Kensington, observing with a 6-in. achromatic by Cooke, has seen Schroeter's crater as a bright spot on the crest of the wall, when the morning terminator had advanced to longitude 84° 30′. The history of this object shows that Schroeter's Selenography is not absolutely obsolete and worthless, and it also affords another illustration of the caution that should be exercised before resorting to an hypothesis involving change with respect to minute lunar features of this class, though in this particular instance it must be confessed that the fact of the crater being now so conspicuous an object at sunrise that the most careless observer could scarcely avoid seeing it, while Neison, fully aware of Schroeter's record (see Neison's Moon, p. 315, footnote), evidently searched for it and bestowed considerable attention on the formation as a whole without detecting it, furnishes just one of those cases of suspicion of variability, due to some unknown cause, which seems to warrant further investigation from time to time. Before leaving the border of Marius, I would refer to the valley, resembling in places a crater-row, which runs from the crater on the south-west wall, just beneath the crest of the latter, and, following the curvature of the western border, terminates apparently on the eastern side of a large mountain of no great altitude just outside the north-west slope of the wall, gradually increasing in breadth and depth as it proceeds north. In published drawings Schmidt alone shows this feature, but he omits the component craters of which it appears to be in part made up, drawing it simply as a wide valley or terrace.

In Beer and Mädler's map the interior of Marius is a blank, no object of any description being shown thereon, though Gruithuisen in 1825 had detected a delicate crater on the north-west side of it, which Schmidt seems to have first noted on 6th March, 1849, with the five-feet refractor at Bonn, as very distinct when the E. long. of the morning terminator was about 60°. He subsequently recorded it in the same terms on 16th January and 13th February 1851, with the terminator lying in the first instance 33°, and in the second 14°, east of the ring-plain; and more recently, on 23rd September 1855, 8th January, and 12th February 1865, under very similar conditions as regards the two former dates. Mr. Webb, unacquainted with Schmidt's previous observations, detected it in 1864 and describes it as "conspicuous in the lunar morning, though invisible in the evening" ('Celestial Objects,' 4th ed. p. 111). In Mr. Birt's 'Selections' (1st issue) there is a brief note on Marius by Mr. Gaudibert, where he records the observation, on 11th January 1873, of the crater as a very white object, standing on a somewhat elevated portion of the interior, which "is slightly raised like a wide band in the middle from one end of the floor to the other, from the N.W. to the S.E." Nearly central on this raised strip he observed the mound shown by Schmidt and Neison and discovered by Schroeter. On 24th September 1884, the W. long. of the evening terminator being about 78°, though I found the outline of the formation very faint and ill-defined and no indications whatever of the crater on the south-west wall, three definite objects were visible on the floor, viz., the crater on the north-west side of it, a small oval light spot nearly central, and a much fainter spot not far from the south-east wall. At this phase there was a very marked difference in tone between the eastern side of the floor and the region on which the crater and spots were situated, the former being somewhat darker than the surface outside the ring and the latter much lighter, while the western, though less dark than the opposite side, contrasted also with the central light strip and included a dark triangular area extending from the south-west wall to the major axis of the ring. Mr. G. T. Davis of Theale saw these various features very similarly on 5th May 1887. Schmidt and Neison, though recording the crater and central mound, show nothing south of the latter; while Gaudibert draws a small object under the south-west wall, which can hardly be the same as that noted by myself and seen subsequently by Mr. Davis, and also by Mr. D. Smart on Oct. 28th, 1887, with a 10-in. Calver reflector. These as well as other details in connection with Marius call for further examination under as varied conditions of illumination as possible.

In the hilly region on the north, which includes some very curious and noteworthy examples of parallel ridges, two comparatively large craters were remarked on 26th December, the more southerly of which is represented by Schmidt alone, while the other, situated on the summit of a ridge nearly due north of Marius, and at a distance from it about equal to the apparent diameter of the formation, has not hitherto been described. The evening was particularly favourable for searching for the remarkable serpentine cleft discovered among the highlands north of Marius by Schmidt on May 10th, 1862, but no traces were seen of it. When first detected the E. long. of the evening terminator appears to have been about 55°, but it was seen subsequently, on Nov. 30th, the same year under a solar altitude of 15°.

Kempston, Beds, Jan. 20, 1888. THOMAS GWYN ELGER.

The 'Nautical Almanac.'

IN 1830 the Admiralty requested the advice of the Royal Astronomical Society on the subject of the improvement of the 'Nautical Almanac,' and in consequence a Committee was appointed to consider it. The report of this committee is published in Vol. iv. of the 'Memoirs;' it was adopted, and the Almanac for 1834, which was the first issued on the plan recommended, presents the familiar features which in the main have marked the work since that time. Improvements there have been in various ways, but the book has been the same nearly in matter and arrangement. A comparison with the Berlin 'Jahrbuch' and the 'Connaissance des Temps' of the time shows that, after the changes then made, it was vastly superior in information to the other works.

We have heard so much recently of the advances of the last half-century, that we are, I fancy, nearly all tired of them; but I must ask the patience of my readers if I speak slightly of those which affect the subject of this paper. Steam has shortened voyages and the courses have become far more under command from port to port; meanwhile chronometers and our knowledge of them have increased, and many a ship now goes to the Antipodes without using any means of checking its time-keepers beyond the time-signals at ports of call whose longitude (thanks to electricity) is well known. The common use of the equatoreal, which was in its infancy, has made known to us well on to 300 small planets, and the number is constantly increasing; comets, too, are observed with a frequency and accuracy which were unknown. The necessity thus arises, far more than in those days, of computing orbits, reckoning the effects of perturbation, and supplying ephemerides. Observatories have multiplied, public and private, and astronomy has a strong hold in distant colonies which look to the 'Nautical Almanac' for information on all astronomical matters. How has this National Ephemeris kept pace with all these demands? and what could be done to make it more useful? These are questions which it seems to me it is time to consider, and in this belief I have asked the Editors of the 'Observatory' to open their pages not only to my views, but to their discussion, and the suggestions of others who may be more competent, each in his own line, to speak of our wants.

I propose, then, to consider each body or group of bodies which is mentioned, or class of observations which require data from the 'Nautical Almanac,' in succession; but before doing so, I would offer the following rule of judgment which seems to me almost axiomatic. When an element given in the 'Nautical Almanac' is directly determinable by observation, then it should be given in such a way that only correction for instrumental errors and for the position of the place of observation will enable the comparison to be made. In other cases the elements given should require as little modification as possible before being turned to account.

First, then, comes the *Sun.*

The places given in page 1 of each month are especially useful at Greenwich and in England. It seems that experience has shown that the data in this page and page 2 of each month are generally in convenient forms. I would only suggest that possibly the omission of the equation of time in page 2 would, without inconvenience to any one, enable the variations of Right Ascension and Declination for an hour of Mean Time to be shown.

As regards the Sun's Longitude and Latitude, it would be better to give these free of aberration and referred to the mean equinox of the beginning of the year. There are very few cases, I fancy, where they are not corrected to this origin before being used.

Then as regards the Solar Coordinates, these are given to the true equinox of date with a correction to the mean equinox of the beginning of the year. Now in using them in computing an ephemeris, we require the latter value, and if both are wanted, then it seems to me that the mode in which they are given in the French ephemeris is the best, where the values to the mean equinox are the principal ones and the correction reduces to the true equinox. But it would be preferable to omit the latter, following the practice of the 'Jahrbuch,' to give corections to the mean equinoxes of the nearest tenth year, or, if possible, those to both the preceding and following tenth years, which in some cases would be very convenient.

The Moon.

I think two places should be given in the parallax as in that of the Sun, and that it would be very convenient if, as in the 'Connaissance des Temps,' the values could be given for every six hours. The right ascensions and declinations of course are required for each hour; but I doubt the value of the longitudes and latitudes. The intervals are now too great for convenient interpolation if they are wanted; and that this is tolerated shows, I think, that they are not wanted except to such an accuracy as will suffice for computing the small terms in nutation.

Lunar Distances are still necessary for seamen, but the number of vessels where they are never taken is very great and is probably increasing. On shore they may well be superseded by the method of equal altitudes of the Moon and a known star. That they hold their ground is probably largely due to the fact that naval men are the principal instructors in geographical astronomy; but every one who has computed results from them is aware that they are most unsatisfactory.

The next reference to the Moon is in the Moon culminating stars. If an ephemeris of the Moon at transit is desired, then I should like to see the plan of the 'Connaissance des Temps' followed, where the equatorial coordinates are given at transit over meridians an hour apart. Except near Greenwich or the Antipodes, the scant information as to the Moon's right ascension is nearly useless. Nor is what was known as the method of Moon culminating stars now in any practical use. A reference to the description of it by Mr. Baily in Vol. ii. of the 'Memoirs' will show that it was proposed by referring the Moon's place to stars near it to avoid the necessity of accurate knowledge of the errors of the Transit instrument. No one now would, I fancy, follow such a procedure; but the Moon would be referred to the best zero points, which should be taken from a list of Standard Stars.

Occultations and Eclipses I propose to consider together later.

Planets.

The Ephemerides in Right Ascension and Declination and the opposition Ephemerides should, I think, stand next. I would suggest that the Heliocentric Latitudes and Longitudes &c. should be replaced* by Heliocentric Coordinates referred to the mean equinox of the beginning of the year, with corrections as in the case of the Sun, or that these data should be furnished in addition for Venus, Mars, Jupiter, and Saturn, at all events, the object being to facilitate the calculation of special perturbations.

As regards the small planets. We now have opposition ephemerides for the four older ones; the rest are left to the Berlin 'Jahrbuch.' It is clear that if these are taken up the cost would be considerable, and even if the Government bore the expense it might be desirable to publish them separately rather than increase the size and cost of the present work.

Stars.

It seems to me that the number of stars whose places are given might be greatly increased with advantage, Of course there is the consideration that the places should be *good.* But the 'Connaissance des Temps' manages to find 300 and the 'Jahrbuch' 622 sufficiently good. I would point out that in almost all Private Observatories and in Scientific Expeditions &c. it is very desirable that the determination of clock-error should not be too long an operation. At the same time it seems that, looking to the smaller stability of the instruments, the azimuth as well as the other errors should be frequently determined. Therefore I should be especially glad to see a larger selection of stars near each Pole. Of time stars, there must be a large selection which have been observed with the moon &c.

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* There seems to be more objection than I had anticipated to omitting the Polar Coordinates.

A matter of minor importance, perhaps, is the time for which apparent places should be given and that for reduction constants. These last I would give for the beginning of the day (whether astronomical or universal), like all the other data which are only given once; and it seems to me that the Places of Stars might be for the same time, with a gain in the computing office and no practical loss; certainly none when the meridian of observation is considerably different from that of Greenwich. The terms depending on the Solar Perigee should be included in the constants.

Occultations and Eclipses.

I now come to these occasional phenomena. First as regards *Occultations.* Of course in the case of those visible at Greenwich no change need be made; but as regards the Elements for Prediction, I would strongly urge, as I did in 1868, their being presented differently. The simplest arrangement is that of the 'Jahrbuch,' the most complete that of the 'Connaissance des Temps.' The arrangement of the 'Nautical Almanac' is compact but inconvenient for computing; but the data of the American 'Nautical Almanac' are as compact and sufficient for convenient prediction, and they might possibly suffice. That our mode of giving these elements is singular seems to me a strong argument for believing that the other is more generally convenient.

As regards *Eclipses.* If the change above be made in Occultations then probably Eclipse Calculations would be conducted on similar formulæ, and it would be desirable to give Elements as in the 'American Nautical Almanac,' which would be the foundation of the calculated quantities for the Earth generally. Otherwise the elements would remain as now.

Miscellaneous Matter.

We have heard a good deal of the defects of the predicted Phenomena of Jupiter's Satellites. Of course the Superintendent of the Almanac can only publish such predictions as the best Tables at his disposal can give, and the information given produces observations which can be used to perfect the Tables and improve our knowledge of the system.

No one, I suppose, uses the Eclipses of Jupiter's Satellites for longitudes, unless for very rough preliminary determination, and this reason for placing them in the 'Nautical Almanac' falls to the ground. Every other seems to me to call as forcibly for Ephemerides of other satellites, such as the 'Observatory Companion' and the 'Monthly Notices' contain, or such as may be seen in the American 'Nautical Almanac.' I suppose no possible selection would satisfy every one, and I will leave the question of what should be given to others.

Arrangement.

This is a very important matter in an Almanac. I suppose no one has had occasion to use ours without feeling that for many purposes the plan in use is inconvenient, and if he examines other ephemerides he will find that general consent is not in its favour.

In the first place, I think that all matter likely to be of use to only one class of men should be separated from what is of more general use. If possible it should be in separate sheets: it might even be desirable that it should be in a separate cover, but I see strong objections to this. Then I think that, subject to the above condition, all matter concerning one body should be collected for the year.

Thus I would collect all Apparent Right Ascensions and Declinations for transit at Greenwich together, placing those of the Sun first, then those of the Moon, and then the Planets successively, closing with the Occultations at Greenwich.

In the more general ephemeris I would follow the same order. It would contain the Apparent Right Ascensions and Declinations of the Sun for the commencement of the day, then those of the Moon for each hour, and possibly for Transit over Meridians separated by an hour of longitude. Then would come those of the Planets and Stars for the beginning of each day, Elements of Occultations and Eclipses, &c.

Then would come a Computer's part: Longitudes and Latitudes and Distances of the Sun, Coordinates of the Sun and Planets, Tables for correcting star places from mean to apparent places and *vice versâ*, and other tables specially useful to computers. Here would come the Latitudes and Longitudes of the Observatories, which might give names of Directors and references to their publications and the places where early publication is made of observations.

And, lastly, would be the Lunar Distances, Tide Tables, &c., which are more especially meant for nautical use.

Those who will examine the Ephemerides will see that the modern 'Connaissance des Temps' nearly represents what I have suggested. If it do so, it is not that I have followed its guidance in forming my ideal: as a matter of fact I have very recently become acquainted with it in its present form, and my ideal has been formed from the experience of years, far fron libraries or others of similar pursuits. Were I now away from England I should certainly choose the 'Connaissance des Temps' for general use in preference to any other ephemeris I know, and I am far from sure that even in England it would not be the best single book of reference, except near Greenwich. I only hope that we may soon have given us an English Annual which shall suit as well the wants of the English-speaking people scattered over the whole surface of the Earth in every longitude.

J. F. TENNANT.

[There would not seem to be much advantage in giving the variations of R.A. and Dec. of the *Sun* on both page 1 and 2 of each month. The figures would be practically the same. With reference to moon-culminating stars, it is doubtful whether even the most careful modern determinations of the errors of the transit-circle are not still advantageously supplemented by observations of the culminators. We have examined the year 1872 of the Greenwich observations, comparing the places of all the culminators as observed on any night with their catalogue places, and the residuals though small agree *inter se* on each day, showing that the correction indicated is really applicable to neighbouring observations. As to the number of standard stars, it is probable that many might be found of sufficiently well-determined places to use in private observatories and scientific expeditions; but the continuous observation to which the stars in the present list are subjected at Greenwich is gradually defining their places to a yet higher order of approximation; and it is to be remembered that large additions to their number would seriously increase the difficulties of getting every star well enough observed for modern requirements. It might be advisable therefore to keep a supplementary list such as Gen. Tennant suggests on a somewhat separate footing.—EDS.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Rotation of Jupiter.

GENTLEMEN,—

I have no wish to dispute Mr. Denning's view "that it seems far easier to consider the red spot an atmospheric phenomenon, than that the planet is subject to remarkable fluctuations in its rotation period." What led me first to write upon the subject was the way in which Mr. Denning, in his communication to the 'Athenæum,' seemed to consider any other view as unworthy of attention, on account of the enormous size of the giant planet, more than 1300 times the bulk of our own, which made it impossible, in his opinion, to accept a change (other, I presume, than very trifling) in its rotation period. This way of putting it appeared to me to ignore the known facts that, though the globe of Jupiter is much larger than that of our Earth, its density is much smaller, and its rotation much more rapid. I am, of course, acquainted with the early observations of Cassini and others, but I scarcely think their accuracy is likely to be sufficient to be inconsistent with the theory of a change of rotation, amounting to a fraction of a second in a year. Undoubtedly, however, the *irregularity* in the lengthening period of the red spot suggests that atmospheric effects must be concerned in the phenomena presented by it. If I might venture to offer an opinion on its nature, it would be a compromise between the views of Mr. Denning and Prof. Hough, viz., that the outline of the red spot (or the exterior portion nearest to us when we see it) is atmospheric, but that its persistence is determined by the existence of some notable feature on the actual body of the planet below, similarly to the way in which clouds are apt to gather round the summit of a mountain on the Earth. An atmospheric drift in one direction might thus well cause an apparent change in the rotation of the whole spot, unless we had the means of fixing on the part which is immediately above the actual surface of the supposed elevated part of the planet. This we have not; but if, after a few more years, the rotation of the red spot appears to be accomplished in the same period as when it was first observed, great probability will be given to the view here advocated.

Yours faithfully,

Blackheath, 1888, Jan. 4. W. T. LYNN.

Magnitude of Sappho.

GENTLEMEN,—

When I published an opposition ephemeris of Sappho last year the calculated magnitude given by me was 10·8.

Dr. Lohse at Scarborough first found the planet, and estimated it at 9·8 mag. on Argelander's scale. He subsequently estimated the mag. as 10.

Col. Tupman at Harrow also estimated the planet as then of the 10th magnitude.

Consequently the calculated magnitude above given requires a correction of −0·8 mag.

The calculated magnitude for the opposition in 1889 is 9·2. If we apply the above correction, the magnitude becomes 8·4.

Last month I received from Dr. Gill at the Cape of Good Hope a letter indicating that in his opinion Sappho would be too faint in 1889 for heliometric measurements for determining the solar parallax. I wrote, mentioning the above fact in reference to the last opposition. In reading a paper in connection with this subject at the last meeting of the Royal Astronomical Society, I intimated that Dr. Gill would not observe Sappho in 1889. I was unaware that at that very moment a telegram was on its way to me from Dr. Gill saying, "statement about magnitude of Sappho changes matters entirely. Planet will be observed in eighty-nine. Send me approximate ephemeris for that year as soon as possible."

I shall be glad if those who are able to observe the planet at the next opposition will publish their results as early as possible or transmit them to me.

London Institution,
1888, Jan. 17.

Faithfully yours,
Robert Bryant.

Maxima and Minima of Variable Stars.

Gentlemen,—

I have the honour to send you the dates of maxima and minima of variable stars observed here in the years 1886 and 1887 compared, when possible, with the predictions of the "Companion" to the 'Observatory.'

Star.	Max. or Min.	Observed Time.	Calculated.	C−O.	Mag.
		d h m	d h m		
U Cephei	*m.*	1887, Jan. 26 10 45	Jan. 26 10 21	-24^{m}	9·4
	m.	Feb. 25 8 34·5	Feb. 25 8 17	$-17^{m}\cdot5$	9·4
Mira Ceti	M.	1886, Nov. 30	......	......	4·3
	M.	1887, Nov. 14	Nov. 2	-12^{d}	4·55
S Orionis	M.	Jan. 29	Nov. 1	−89	8·8
U Geminorum	M.	1886, Mar. 9	......	......	9·4
	M.	Dec. 1	......	......	9·4
	M.	1887, Feb. 23·3	......	......	9·6
R Ursæ Maj. .	*m.*	1886, Nov. 29	......	......	13·5
	M.	1887, Feb. 21	Mar. 11	+18	8·05
	m.	Sept. 13	Aug. 5	−39	13·35
S Ursæ Maj. ...	M.	1886, Aug. 10	July 24	−17	7·7
	m.	Dec. 2	Nov. 14	−18	13·05
	M.	1887, Mar. 13	Mar. 10	− 3	7·9
	m.	July 11	July 1	−10	11·5
	M.	Nov. 1±	Oct. 25	− 7	7·8
R Scorpii	M.	1886, July 28	July 31	+ 3	9·8
S Cygni	M.	Sept. 19±	Sept. 24	+ 5	9·7
	M.	1887, Aug. 11±	Aug. 13	+ 2	10·4
S Aquilæ	*m.*	1886, July 7	Aug. 7	+31	11·0
	M.	Oct. 4	......	......	9·2
	m.	Dec. 2	Jan. 1	+30	10·75
	M.	1887, July 10	......	......	9·2
	M.	Nov. 23	......	......	9·2
U Cygni.........	M.	1886, Aug. 17	Sept. 20	+34	7·9
	m.	1887, May 31	May 13	−18	10·6
R Vulpeculæ ...	*m.*	1886, July 5	Sept. 3	+60	13·6
	m.	1887, Aug. 21±	Sept. 30	+40	12·8
	M.	Oct. 20±	Dec. 5	+46	7·8
T Cephei	M.	1886, Mar. 26	Mar. 14	−12	6·3
	m.	Oct. 16	Sept. 20	−26	9·6
	M.	1887, Mar. 26	Mar. 31	+ 5	6·3
	m.	Nov. 14	Oct. 7	−38	9·2

Mira Ceti.—The maxima of 1886 and 1887 were rather low in magnitude. In February 1885 the star reached 2·9 mag. While observing the max. of 1886 I noted the star ξ^2 Ceti to be brighter by one or two tenths than ξ^1. The mags. assigned by Prof. Pritchard are—for ξ^1 4·51, and for ξ^2 4·66. Prof. Pickering's values are—for ξ^1 4·45, and for ξ^2 4·41. Perhaps ξ^2 is slightly variable.

U Geminorum.—A maximum of this star occurred on January 2 or 4 in the present year. The period is very irregular.

S Ursæ Majoris.—A well-marked subsidiary maximum on the rising curve was noted on Feb. 6, mag. 8·8. On Feb. 11 the star had fallen to 9·0 mag.

S Cygni.—In the 'Observatory' for August 1884, I pointed out an irregularity in the period of this star, which seems to have attained a maximum in 1881. The discordance between the observed dates of max. and those calculated from Prof. Schönfeld's elements has declined since that year, when it amounted to +30·3 days, and was only +1·9 in 1887.

R Vulpeculæ.—I have assumed December 5 as the date of a *maximum*, not of a *minimum*, as printed in the "Companion" for 1887.

Yours faithfully,
GEORGE KNOTT.

Knowles Lodge, Cuckfield,
Haywards Heath, 1888, Jan. 13.

[WE have received a further letter from Prof. Zenger, attempting to show that the periodic times of the asteroids are connected with the half-period of rotation of the Sun $\left(\frac{T}{2}\right)$. He forms residuals by comparing 259 of these periodic times with multiples of $\frac{T}{2}$, and takes the *algebraic* mean of the groups referring to the same multiple; and bases his conclusions on the fact that these means are on the whole less than $\frac{T}{8}$ (see 'Observatory,' No. 129, p. 395). We need hardly point out that this is a thoroughly vicious process. The residuals referring to the same multiple are liable to range indifferently, if the distribution of periodic times be purely accidental, between $-\frac{T}{4}$ and $+\frac{T}{4}$, and the *algebraic* mean would in any case be a small quantity. The mean without regard to sign, if any, should have been taken; but it is more satisfactory to take the original series of residuals. With Prof. Zenger's figures we find that these are grouped arithmetically as follows, expressing them in terms of $\frac{T}{2}$:—

From ·0 to ·1	41	·3 to ·4	60
·1 to ·2	56	·4 to ·5	60
·2 to ·3	42		

that is quite indifferently. His figures thus prove exactly the opposite of what he supposes.—EDS.]

The Chromosphere in 1887.

The entire chromosphere has been measured 123 times during the twelve months, and partial observations have been made on three other occasions. The following are the mean values obtained, and are arranged as in previous years:—

	No. of days of observation.	Mean height of chromosphere excl. prominences.	Mean height of prominences.	Mean extent of prominence in arc.	Highest prominences.	No. of days on which C was displaced.
		″	″	° ′	′ ″	
January ……	5	8·12	22·81	12 9	0 53·41	0
February ……	14	8·16	25·46	10 21	1 22·88	1
March ………	17	8·07	20·76	9 32	0 55·54	2
April……….	15	8·16	24·86	8 30	1 9·63	0
May ……….	6	8·16	25·42	10 50	1 6·22	1
June……….	18	8·07	20·93	7 33	1 10·06	1
July ……….	13	8·16	24·99	12 2	1 22·43	1
August……..	7	8·16	22·90	9 46	0 46·99	0
September ….	7	8·16	24·73	8 19	0 49·55	0
October ……	8	8·12	27·13	5 47	1 6·22	1
November ….	7	8·07	25·25	11 7	1 7·92	1
December……	5	8·12	21·10	7 55	0 39·30	1
Mean for 1887..	10·2	8·13	23·86	9 29	1 2·51	0·75
„ 1886..	8·4	8·05	24·78	13 36	1 12·77	1·6

Comparing this year with the last, we see that the mean height of both chromosphere and prominences has remained almost constant, although the prominences have decreased very much in number. The height of the largest prominences is also much diminished; and there are not half so many cases of distortion this year as last.

During the years 1886 and 1887 a series of drawings have been made of the chromosphere and prominences with a wide tangential slit, with the view of ascertaining the direction of the surface-currents on the Sun. The direction of the lesser prominences are easily detected; but the filaments of the chromosphere require a very steady atmosphere, as a slight change in the atmospheric conditions is sometimes found to be sufficient to reverse the apparent drift. Twelve prisms of 60° were always used in these observations, and the tangential slit was moved round the solar circumference by rack and prism, the heliographic latitudes being read on the position-circle. A large number of observations must be discussed before any very definite results can be arrived at with certainty: but a first examination seems to indicate the following approximate conclusions:—1. That the direction at the Poles is generally vertical to the limb. 2. That there is a decided current crossing the equator, sometimes in a northerly and at other times in the southerly direction. 3. That changes of direction occur most frequently in mid-latitudes. S. J. Perry.

Stonyhurst Observatory, 1888, Jan. 9.

The Council of the R. A. S.

In answer to Captain Noble's letter of last month, we give the attendance of members of the present Council, and of those proposed for election this year who have been members of the Council at some time during the past three years. The months of January and February, 1887, are not included in the count, which commences with March in each case.

Those for whom the maximum number of attendances is 22:—

Prof. Adams	7	Dr. Huggins	11
Mr. Capron	14	Mr. Knobel	22
Prof. Cayley	7	Mr. Maunder	18
Mr. Christie	20	Mr. Ranyard........	22
Mr. Common........	22	Mr. Stone	20
Mr. Dunkin	22	Gen. Tennant	20
Mr. Glaisher........	15	Col. Tupman	21
Mr. Hind	4		

Maximum 16.

Mr. Downing	12	Mr. Knott	13

Maximum 14.

Capt. Abney	8	Lord Crawford	1
Mr. Chambers	9	Capt. Noble	13

Maximum 8.

Dr. De la Rue........	7

Maximum 6.

Rev. S. J. Perry	3	Mr. Turner	6

Eds.

We have received the following letter on this subject:—

Gentlemen,—

I see you are going to publish next month a list of the attendances of Members of the R.A.S. Council. That is well as far as it goes; it is quite certain that regular attendance and interest in the affairs of the Society are qualifications for a seat, and that neglect to attend is not a recommendation.

I would, however, as an appeal is virtually being made to the

Fellows at large, point out that our List is large (many might think too large), and that some seats are well devoted to men of eminence, who from various causes are not able to attend meetings regularly, but who aid us with advice and assistance when it is required. It is to be hoped that Capt. Noble's letter may not be understood to mean that such Members are useless, and I do not believe that he would wish such a conclusion to be drawn from his words.

From one cause or another we only too often miss from our list the names of men whom we have known personally or by repute, as long as we have been interested in our science; and I should be sorry to think that the Society was ready to cast off such men as soon as their attendance at Council meetings becomes scant. The weight of the Council's decisions and the honour in which the Society is held would not be increased by the absence of their names, nor would the current business be better done if they were replaced.

Yours faithfully,
J. F. Tennant.

37 Hamilton Road, Ealing, W.,
1888, Jan. 3.

The Total Eclipse of the Moon.

The weather cannot be said to have been generally good on Jan. 28th, though in one or two favoured spots it was beautifully clear. At Dunecht Dr. Copeland was almost entirely baffled in his spectroscopic observations by snow-squalls. At Halifax (Mr. Crossley's Observatory) and Vienna it was quite cloudy; at Berlin, Paris, and Madrid it was partially clear, and observations were made: at Moscow and Dublin very clear. We have received the following details from other places, some of them being in response to a request which the editor of 'Nature' kindly inserted in last week's number.

Place.	Instr.	Observer.	Number of Obs.		Remarks.
	in.		Disap.	Reap.	
Bath	6	J. L. Stothert	11	13	6 pairs: very fine.
Bradford	6	Miss Petrie	1	1	Pair: fine.
Cambridge	...	A. Graham	0	2	Clouds and snow.
Dunsink	...	A. A. Rambaut	17	18	10 pairs: very fine.
Fern Hill, I. of W.	8½	F. Brodie	9	5	4 pairs: fine till near end.
Glasgow	9	R. Grant	6	7	Fine: 1 star not in list.
Greenwich	24*	H. H. Turner	3	7	Cloudy, with occasional breaks for a few seconds only. Three photos taken of partial phases.
„	12½	E. W. Maunder	1	4	
„	6½	T. Lewis	0	3	
„	6	H. P. Hollis	3	1	
Harrow	18*	J. F. Tennant	3	2	Very cloudy from 10^h 50^m to 12^h.
Stonyhurst	8	S. J. Perry	1		The 8-inch used also with spectroscope: clear.
„	5½	....	12		
„	4	....	4		
Wimbledon	...	F. C. Penrose	3	2	2 pairs: clear at times only.

* These instruments are reflectors; the others refractors.

NOTES.

Measurement of Stellar Photographs.—The literarature of this subject will no doubt soon be voluminous. In a contribution to it, published in the Ast. Nach. No. 2818, Dr. Scheiner, of Potsdam, discusses a few measures of a plate by the Brothers Henri, on which three exposures of 4^m, 2^m, and 1^m respectively are in juxtaposition. He finds the probable error *decrease* with length of exposure, in the ratios 14 : 11 : 10 for the above durations, and 7½- to 8½-mag. stars. He finds also that the length of exposure has little or no influence on the star's place; but there is not enough range in star-magnitudes or in time of exposure to show this satisfactorily, nor is any information given as to the distance of the stars from the centre of the plate—a most important element.

Transit of Venus, 1882.—Prof. L. Cruls has published[*] the result of the Brazilian observations at the three stations—St. Thomas (Antilles), Pernambuco, and Punta-Arenas. The final result for parallax is 8″·808; probable error not given. He mentions as a curious fact that the result of the English observations is 8″·832 ±0″·024, which is coincident with the Brazilian result if the lower sign be taken.

[**The** Astronomer Royal kindly allows us to print the following extract from a letter addressed to him by Prof. E. S. Holden.]

"* * * * We expect to begin work at Mount Hamilton in January; and I shall then be permanently there, as I have resigned my other duties. The mounting of the large telescope is being hauled up the mountain to-day, and we expect the photographic corrector by Dec. 14. Everything else is quite ready. I can shortly send you Vol. I. of our Publications, which gives a description of the Observatory, with such observations as have been made in the years 1881–85, and full tables of reduction &c."

Dec. 5, 1887.

We are glad to notice another indication of the prosperity of the Liverpool Astronomical Society in the Catalogue of their library, lately printed. The number of volumes is already nearly four hundred.

Prof. Oudemans, of Utrecht, is engaged in deducing new elements of Algol, and he requests that observers of Algol's minima will kindly send him their results, especially those obtained since 1883.

The 'American Journal of Science' for January contains an interesting article by Prof. Newcomb and Mr. Dutton on the speed of propagation of the Charleston Earthquake as determined by clocks stopped along the route. The result in metres per second is 5184 ±80. "The data are inconsistent with any variation in speed of a systematic character, and there is no apparent means of detecting an unsystematic one."

Prof. S. P. Langley has been elected secretary to the Smith-Institution, as successor to the late Prof. Baird.

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[*] 'Revista do Observatorio do Rio de Janeiro,' Nov. 1887.

THE TOTAL SOLAR ECLIPSE OF 1887, AUGUST 19.—Three photographs of the corona were obtained at Yomeiji-yama, Echigo, Japan, by Prof. Jarai and Mr. M. Sugiyama of the Tokio Observatory. The times attached are $1^m\ 8^s$, $1^m\ 57^s$, and $2^m\ 28^s$ after the commencement of totality; but the exposures are not given on the sheet of silver prints which has reached Greenwich. Judging from these prints the negatives should be very good indeed. The Moon's diameter is 1·7 in., and the corona shown extends in the first print at least one diameter from the limb, the size of the print limiting it there. It is possible that the negative is more extensive. The prints agree remarkably well with the cut shown on p. 92 of the 'Observatory,' No. 131, as obtained by Dr. Handrikof. The long ray just mentioned is that lettered A in his figure. Some furthur particulars of the prints are added, but our ignorance of Japanese is a fatal obstacle to their communication.

SATELLITE GENESIS.—In a paper[*] recently communicated to the Royal Society, Prof. Darwin considers the possible forms of equilibrium of two masses of fluid, which revolve in close proximity (and finally in actual contact) about one another without relative motion of their parts. At a considerable distance their deformations from a spherical form would be expressible by a series of spherical harmonics, of which only the first few orders would be sensible. At smaller distances, however, the higher orders cannot be neglected; and the summation of an infinitive series must be considered, and the departure from spherical shape becomes so important that the question arises whether spherical harmonic analysis is adequate to the solution. This point the author considers with a satisfactory result. He then discusses numerically the cases of—

(1) Equal masses of fluid, which he follows until they conglomerate into a dumbbell shape.

(2) Unequal masses, for which there is a limiting distance between the centres for stable equilibrium, depending on the ratio of the masses. Thus an infinitely small satellite could not be revolving as one body with its primary unless it be at least two radii distant; but a satellite of the mass of the Moon could approach till the distance between centres was 1·6 radii of the Earth, when the surface would be only 380 miles distant, and a satellite $\frac{1}{30}$ of the mass of the primary could revolve in actual contact with it. From the discussion of a case of equilibrium it would appear that the tendency in the unstable cases is for the satellite to break up by fission: in which case it would seem that the parts would have a still stronger tendency to break up; and it does not seem impossible that this process might be continued almost indefinitely. Indeed, if a ring be a swarm of meteorites, it may be the result of such a series of breakings-up.

Prof. Darwin does not, however, allow himself to speculate in this manner. In referring to a memoir of M. Poincaré's* on the same subject (in which, however, the inverse process is followed), he endorses his opinion that the results obtained for liquid masses are not of much help in suggesting the history of nebulous masses condensed towards the centre.

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[*] Phil. Trans. Vol. 178 (1887) A, pp. 379–428.

THE AMERICAN NAUTICAL ALMANAC.—Prof. Newcomb's report on the American Nautical Almanac for the year ending 1887 June 30 has been published. He ascribes the diminution in the sales, which continues from year to year, to the diminution in the number of American ships engaged in foreign trade. We do not feel sure, however, that it is not in part due to the increasing number of cheap handy tables, pirated from official works, which thus have the same accuracy without being burdened with much that the ordinary merchant-captain never uses, though it is probable that with a little care and judgment he might find the despised portions of great service. We were very glad to learn the other day, for instance, that an enterprising officer of the mail-steamer 'Nile,' which conveyed the Eclipse Expedition of 1886 to the West Indies, has since (in his new capacity of captain) taken to using occultations for determining his longitude, though of course he does not find them occur often enough to satisfy all requirements. On another page of this number this subject is more fully discussed.

Prof. Newcomb gives a detailed report of the work now proceeding for forming new tables of the planets. The work is divided into four sections, viz.:—First, the computation of the general perturbations of the planets. Second, the re-reduction of the older observations and the discussion of the later ones, with a view to reducing them all to a uniform system. Third, the computation of tabular places of the planets from Leverrier's Tables up to the year 1864. Fourth, the final discussion of all the results.

The work is well advanced in the first three sections for the four inner planets, and it is hoped to have the equations of condition for correcting their elements ready by the end of 1889. Mr. Hill's new theory of Jupiter and Saturn is being printed; but on Uranus and Neptune nothing has been done. The comparison of lunar occultations since 1750 with Hansen's tables, and a determination of the mass of Jupiter from the motions of Polyhymnia are also in hand.

PULKOWA OBSERVATORY.—We have received the report of this Observatory for the year ending 1887, May 31. It opens with a reference to the great loss sustained by the Observatory and the world in the death of August Wagner; and those who were so fortunate as to visit Pulkowa in August last can bear witness to the true greatness of a man whose memory they found so lovingly cherished by all who knew him.

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* Acta Mathematica, vii. 1885, p. 379.

The work of the Observatory has for the most part proceeded on the same lines as before. 3767 transits were observed with the transit instrument and 1130 zenith-distances with the vertical circle, each of these involving two observations in reversed positions of the instrument. Prof. Struve considers this the most satisfactory method of obtaining fundamental declinations, and prefers to use the transit-circle only for differential measures. With this latter instrument Herr Romberg had, in ten months, observed 5900 stars; and, as we were told in August, he had increased this to the magnificent total of 9000 observations in the year. We should imagine that such a year's work has seldom if ever been credited to a single observer. With the 30-in. refractor Dr. Hermann Struve is measuring those of Burnham's double stars which are only seldom measurable with the "old 15-inch," together with other stars of which measures are scarce, making a working catalogue of 750 stars. He made 460 measures in eight or nine months, as well as 166 micrometric observations of the fainter satellites of Saturn, and 15 of that of Neptune. The Director has only twice used his own particular instrument—the "old 15-inch" —for observations on Procyon, which he has now followed through nearly a complete period of its orbital motion; but the instrument has also been used by H. Struve, and by Hasselberg for some experiments on stellar photography.

The "Astrophysical" laboratory is now in good working order under Herr Hasselberg, but owing to various interruptions during the last year no systematic investigations could be undertaken.

Wagner's death has been especially felt in the delay of the publishing. He lived to complete a memoir on instrumental and personal errors, which forms the introduction to vol. xii., but the material for the stellar and planetary catalogues left among his papers could not be so readily worked into shape by another; it is hoped, however, that with Herr Nyrén's zealous exertions the volume will soon be published, as also the introduction to vol. xiv. The outstanding volumes viii. and x. are not so well advanced; the Catalogue forming the first part of viii. (meridian observations 1840–1869 of Bradley's stars and others to the sixth magnitude) is already distributed, and progress is being made with the rest of the volume; pressure of other business has prevented the Director completing vol. x., which contains his own observations of double stars. The MSS. of xv. and xvi. only awaits the passing of xiv. through the press, and will contain meridian observations 1872–1880, and the Catalogue.

The members of the staff have shown activity in various other directions. We may specially mention the publication by Dr. Döllen, now Vice-Director, of star-ephemerides, by which the time can be quickly ascertained with a portable transit in the vertical through the pole star. Several of the visitors in August had an opportunity, through his kindness, of appreciating the facility and accuracy of his method.

Ephemeris for Olbers' Comet, 1888 *.

For Berlin Midnight.

1888.	R.A. h m s	Decl. ° ′	Log *r*.	Log Δ.
Feb. 3..	17 36 17	5 16·3	0·3137	0·3969
7..	41 36	5 41·7	0·3224	0·3974
11..	46 35	6 5·7	0·3320	0·3974
15..	51 15	28·4	0·3394	0·3970
19..	55 36	6 50·0	0·3477	0·3962
23..	17 59 36	7 10·7	0·3558	0·3951
27..	18 3 15	30·7	0·3638	0·3936

γ Coronæ Australis.

GENTLEMEN,—

In your notes in the 'Observatory,' No. 129, you draw attention to this interesting binary and express a wish that all unpublished observations of the star might be supplied without delay. The only measures which I possess are those for September 18, 19, 20, and October 8 of the present year. They were made with the 8-inch equatoreal with a power of 300. The definition, more particularly on October 8, was good, but the measures of distance were difficult. The following are the results:—

Epoch.	Position-angle.	No. of Measures.	Distance.	No. of Measures.	Hour-angles. West.
	°		″		h m h m
1887·71	197·57	15	1·68	8	1 28 2 2
1887·71	197·77	15	……	……	0 59 1 35
1887·72	194·69	12	……	……	2 27 2 50
1887·77	194·65	10	……	……	4 16 4 27

The components were equal (5½ mag.). The last column gives the hour-angles between which the measures were made. A comparison of these measures with Mr. Gore's ephemeris in the 'Monthly Notices,' Vol. xlvi. p. 105, will show that the latter does not at present represent the motion of the binary. The position-angle is about nine degrees greater than that deduced from the ephemeris; and, instead of apparently closing up, the components appear to be slowly separating. I trust next year to be able to obtain measures on both sides of the meridian.

Windsor, N. S. Wales,
1887, Dec. 9.

Yours faithfully,
JOHN TEBBUTT.

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* Astr. Nach. No. 2818.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 134. MARCH. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, February 10, 1888.

J. W. L. Glaisher, M.A., F.R.S., *President*, in the Chair.

Secretary: E. B. Knobel.

The Minutes of the last Anniversary Meeting were read and confirmed.

Mr. Inwards read the Auditors' Report and the Balance-sheet.

Mr. Knobel gave a summary of the Report of the Council. This, with the Balance-sheet and Obituary Notices, will be printed *in extenso* in the 'Monthly Notices.'

The President then delivered an address on the work performed by Professor Auwers of Berlin, to whom the Council had awarded the gold medal of the Society for his new reduction of Bradley's observations; and concluded by presenting the medal to Dr. Huggins, the Foreign Secretary, desiring him to convey to Prof. Auwers the congratulations of the Society on his completion of so great a work, and to express not only their admiration for the manner in which the most refined skill had been combined with the most patient care in its performance, but also their personal acknowledgments for the years of laborious industry in which, with untiring zeal, he had devoted his time and powers to the not unworthy task of applying to the advance of modern astronomy the concluding labours of Bradley's useful life.

Lord Rosse moved that the Report, now read, be received and adopted, and that it be printed and circulated in the usual manner, together with the President's Address and the Report of the Auditors.

Mr. Brett seconded the motion, which was carried unanimously.

The President said that a notice of motion signed by four members had been given, dated January 12th, "That Bye-laws 71 to

75* inclusive, referring to the award of the Society's medal, be repealed." He called on Mr. Ranyard to move it.

Mr. Ranyard said that he had hoped Lord Grimthorpe would be there to move the motion, but he was unfortunately ill at Bath. He (Mr. Ranyard) was sorry to come before the Society year after year with what many considered a vexatious protest, but he felt it to be his duty. He was not adopting new views, for Prof. De Morgan long since expressed himself still more strongly in a lecture in University College in 1848, going so far as to say that he disapproved of all prizes and rewards to boys. What was applicable to boys surely applied to men. Prof. De Morgan's address was circulated, and among others Sir John Herschel and Prof. Baden Powell expressed their approval of it. Prof. De Morgan carried his aversion to marks of distinction to such a length as to refuse to be a Fellow of the Royal Society, which (as he explains in the Preface to his 'Budget of Paradoxes') was obviously a great piece of self-denial to a book-loving man like himself. He considered that such societies should be merely publishing clubs, and everything avoided which might promote jealousy. This was a principle which was universally conceded in private life. Similarly everything should be avoided that promotes flattery—and comparisons, which, according to the copybook headings, are odious. If this was universally admitted in individual life, why should it be set aside for a Society of individuals?

Mr. Ranyard then quoted several instances where he considered the Copley Medal of the Royal Society had been injudiciously awarded, and remarked that Bunsen and Kirchhoff never received our medal. He again remarked that no reasons had been advanced for giving the medal; except perhaps that Dr. De la Rue last year had said that it was both a pleasure and a privilege to do so; but this was no argument. He hoped that those who disagreed with him would point out some arguments. He moved that the Bye-laws referring to the giving of a medal be repealed.

The Rev. T. Harley said he had been asked to second the resolution, and though it might appear a little ungracious to do so just after the touching scene in which the medal had been handed to Dr. Huggins for transmission to its recipient, yet he did second it very heartily. The word "medal" etymologically meant nothing more than a piece of metal, having no significance in itself. Originally a coin, it afterwards became a medallion; and the only two objects of a medal were to *record* and to *reward*. There was no necessity so to record the great events in the history of Astronomy, for the discoveries of Uranus, Neptune, and other planets were recorded in the planets themselves—memorials that would last so long as the Solar system endured. Then it looked to him a little like impertinence to talk of *rewarding* by a medal the work done by those who had devoted their lives to the study of a science like Astronomy. Besides, it had been seen again and again, as Mr. Ranyard had pointed out, that medals had been given for discoveries that had turned out to be of comparatively little value. Nor were the medals themselves much valued. They were like the presentation-books given to boys in schools and colleges, which were valued at about their cost, and were soon afterwards to be found in the shops of secondhand booksellers. In these days a man was a man whether he had the "guinea-stamp" or "hall-mark" or not. Some of the greatest kings were those who had never worn crowns; and some of our greatest men had never received medals. He hoped we had arrived at that period when we did not need such distinctions. They had better put away "childish things." A word to the wise was enough. To quote a passage from Tyndale's Bible, he would add "Brethren, study to be quiet, and to *meddle* with your own business!" (Laughter.)

Captain Noble said that this question was decided at the last annual meeting, but that as Mr. Ranyard's motion threatened to become, as the gardeners would say, a hardy annual [Laughter], he proposed to move in effect his amendment of last year, and he did so because he believed that by pressing it they would cut the ground absolutely from under the feet of those who sought to abolish the award of the medal by removing the solitary tangible objection which could be urged against it. This amendment was "That the Bye-laws 71 to 75* inclusive, referring to the award of the Society's medal, be so altered as to preclude any Fellow of the Society actually holding office or sitting on the Council at the time such award is made from receiving a medal." Conspicuous merit had been rewarded publicly in all ages. They all recollected the triumphs which the Roman generals received on their victorious return to Rome; but no one was foolish enough to suppose that these generals would not have fought as vigorously and thrashed the enemy as completely, even if they had to return in a one-horse chariot or at the rear of a baggage-waggon. He was able to speak candidly on this subject of medals because under no conceivable circumstances could he ever receive a medal himself (Laughter); but he must say that had he been engaged in any astronomical research or investigation and had carried it to a successful conclusion, it would be very pleasant to receive a recognition of that work; and its value would be enhanced if he knew his work had been judged by a jury of experts such as could perhaps not be got together in any other part of the world. Such an award from such a court would take the character of a hall-mark. So far as he was himself concerned, the award would not make the slightest difference to his work, he should work on just the same whether he had received a medal or not. He was convinced that in 999 cases out of 1000 no such thing as a medal ever entered the head of the earnest worker who had a serious object before him. He regarded the medal as a sort of hall-mark. It was like 18-carat gold, which, if not marked, did not command the same price; and therefore the hall-mark did give gold an actual market-value beyond what the quality of the gold, if unmarked, would command.

In order to give the Society's medal its full value, though, the award should be above suspicion, so that no chance should be given to any outsider to carp at it. He did not suppose the members of this Society set up for being more high-minded than the Fellows of the Geological, of the Zoological, or of the Geographical Societies; and yet he was informed by his friend Prof. Wiltshire, who was a member of nearly every learned Society in London, that the members and council and officers of those Societies were precluded by virtue of holding office from receiving any medal. No one knew better than he (Capt. Noble) did the immense pains that were taken in this Society, not only to select the fittest man, but to take care that not the faintest exception should be taken to their award. Like Cæsar's wife, that award should be above suspicion; and in order not to give any one the opportunity of saying that the Council patted one another on the back, they ought to adopt the amendment he proposed. We were all human—some of us very much so—and there was always a temptation, *cæteris paribus*, to give an award to a tried and trusted colleague who had sat by one's side for a good many years, however they might try to fight against the inclination; and that being so, his amendment, if carried, would, he thought, remove the least chance of any accusation or favouritism or affection being brought against the Council of this Society, in the award of the medal.

Mr. Common. It has never been alleged.

Capt. Noble. There is something like that underlying Mr. Ranyard's motion.

Mr. Ranyard. It is a common allegation everywhere. I do not make any personal allegation, but we cannot shut our eyes to the fact that there are people who speak of the medal in that way, and we ought to be above that.

Capt. Noble said, as a similar regulation to the one he now proposed was in force in the leading Societies in the metropolis, he did not see why this Society should not adopt it, and in that way they would be able to root up Mr. Ranyard's "hardy annual" bodily. [Laughter.]

The Rev. A. Freeman, in seconding the amendment, said he was not present at the last Annual Meeting, but had seen a report of it in the 'Observatory,' and he was greatly astonished to find that Mr. Ranyard was bringing forward this year very much the same resolution as was rejected at the last Meeting. He was pleased that Capt. Noble had repeated his amendment, and he hoped that whether it was taken as an amendment on Mr. Raynard's motion or not, it would be a hint to the Council to propose an additional rule with regard to the medal. Capt. Noble's proposition was a most sensible one, and required no argument on his part to support it. He thought it very undesirable that the subject should be repeatedly brought forward, for it would make the Society contemptible in the astronomical world. Something should be done to show that the Society had no sympathy with such a course.

On the other hand, Capt. Noble's suggestion was so reasonable that no one could possibly object to it. The Council could not be attacked this year on personal grounds, for they had given the medal to a foreigner. The way in which the Royal Society had awarded medals in the distant past had nothing to do with the present motion. If there was any complaint of the sort against the Royal Astronomical Society, those who made it ought to make out their case, but he did not expect that they would be able to do so. Their insinuations that the Council had been actuated by motives of friendship did not prove that any person, to whom the medal of the Society had been awarded, was entirely unworthy of it.

Mr. Nightingale thought the last speaker had misunderstood Mr. Ranyard. He seemed to think that Mr. Ranyard had made some charge of bias or malpractice against the Council of the Society. Mr. Ranyard's proposal had a much wider and nobler basis than that; but the full strength of his case could not be learned by anyone who had not read or heard the former discussions. Capt. Noble had played backward and forward in his remarks, yet no one could help thinking his amendment was a good one; the question was did it go far enough? Capt. Noble had answered Mr. Ranyard with the only reason brought forward the year before last, viz.: that it was a pleasure to the Society to give, and to an Astronomer to receive, the medal. That was an extremely fine sentiment, but was it an argument? Mr. Ranyard, on the other hand, put forth cogent arguments against the giving of any medal at all: he had shown, and the Society had admitted, that the award of the medal did not advance science; and even Capt. Noble's argument, that a medal was a hall-mark, really supported Mr. Ranyard's case, because when a man had obtained the highest standard there was no incentive to go further, and he was encouraged to rest on his oars. Again, the award of a medal introduced the personal element and often heart-burnings. Mr. Ranyard instanced at the last meeting a case which nearly broke a man's heart, because the delay in receiving the news of his election to the Academy in Paris led him to think he had not been elected. These matters should not be ignored. They must be answered by argument, if at all; but they could not be answered effectually by getting up, as Prof. Adams got up at the last meeting, and making a clever joke about demoralizing the foreigners. He further alleged that he did not think that Mr. Hill was demoralized by receiving the medal. Mr. Ranyard never argued that the actual recipient of the Astronomical Society's medals was directly demoralized by receiving it, but that the award of medals tended to demoralize men in the mass. When Mr. Ranyard's case was only met by such weak arguments as he had just referred to, there must be something in it. He believed, if stronger arguments could have been used, they would have been brought forward to oppose the motion. It must be assumed, when persons failed to bring forward arguments, that they had none to bring forward. There was only one other alternative, and that was to ignore Mr. Ranyard's motion. But this could not be done. First, on account of the strength of Mr. Ranyard's case, and, secondly, because of the voting of previous occasions. None could deny that Mr. Ranyard had made out a strong case—an almost unattackable one. Anyone who had heard or read the previous debates must see that; and they must have seen how many persons at the last meeting abstained from voting on Capt. Noble's amendment (which was a step in advance—a sort of half-measure). These abstainers, it must be remembered, though they would not vote against the present state of affairs, would not support it with their vote. Under these circumstances they could not treat Mr. Ranyard's motion with contempt, as some persons evidently wished to do. It was only fair that if any serious argument could be brought against it, it should be stated. He agreed that, whilst something might be said in favour of giving prizes to boys, the same argument did not necessarily apply to men. There was a danger that a man would be inclined to trust to his hall-mark rather than to his work when he had once received the medal. The number of prizes—prizes of fortune—had largely increased the number of scientific men; and was it not probable that the same cause was responsible for the depreciation in *morale* that was somewhat apparent in that body as a whole? If that was the case, ought we not to look very jealously at anything that might possibly lead us in that downward direction?

The President said he would put to the Meeting the question whether it would allow Capt. Noble's proposal to take the place of Mr. Ranyard's motion; the question would be—"That the motion be so amended." After counting hands, the President declared the proposal carried.

Mr. Brett and *Mr. Ranyard* challenged the method of procedure.

The President. A motion has been brought before the meeting. It is for the Meeting to say whether they will permit it to be amended or not. If a motion is brought before the Meeting, it is not open to any person to move an amendment without the leave of the Meeting. I trust the Meeting will support me in what I conceive to be the right course. I will take the vote again.

In the result the President declared the proposal carried, and that Capt. Noble's amendment had now become the substantive motion.

Prof. A. S. Herschel said the proposition of Capt. Noble differed very much from the recommendation urged in Mr. Ranyard's motion, and the point of the award itself had been kept out of sight. The award of the medal was a function which the Society had to exercise, and it was intended to form a bond of union amongst the members, connected also with other aims. It was the duty of the Society to award the Medal; in that sense it was not a pleasure; and though Mr. Ranyard urged that it was nothing but a pleasure, he could assure the members that it was no pleasure to award the medal so long as criticism was kept up so lively as at present. The award of the medal was a task for the Society as the gaining of it was a task for the individual. If individuals gained the medal discreditably it would damage the prestige of the Society; but if the Society properly carried out its functions in awarding the medal, it ought to give satisfaction and advance the interests of the Society. Then the medal would be looked upon as a hall-mark, and carry prestige with it; and he agreed that the motion of Capt. Noble, if carried, would be a repudiation of any other motive but a good motive on the part of the Society: and probably some alteration of the Bye-laws as Capt. Noble proposed would answer the purpose.

Mr. Stone said he was sure that the members of the Council who had to make the award of the medal did not look upon it as a pleasure, nor had they any personal motives to serve. He thought they were above anything of the kind; and, if not, it would be impossible for any clique to award the Medal to a mere personal friend from the construction of the Bye-laws. As to the utility of the medal, he did not consider any worker ever thought about the medal, or what had been called the hall-mark; but there was one point in which the medal did good for the advance of the science and for the members of the Council who had to take part in the award. Everyone, as he got on in years, was naturally liable to think his own branch of work of more importance than any other; and it was exceedingly difficult to get people to examine the work of others in a different direction. This was his case; and he was sure it was to some extent the case with others. Here the award of the medal came in, and compelled, as a matter of duty, an examination of work in different branches—an examination which often brought prominently forward work that might otherwise be overlooked or unrecognized; and the judges themselves, being compelled to look into the matters for the purpose of the award, got considerable advantage from their investigation. If they looked back at the past they would see many pieces of work neglected for years, and therefore practically useless for the advance of science, because no one of any considerable authority had had time to look into it and give it a certain amount of acceptance. A hall-mark or medal did give a certain recognition to work done, and brought it forward for use. He did not believe that the medal could be given unworthily; the Bye-laws were framed to make such a thing impossible. The compelling the leading men to examine critically the work of other people was a great thing for the advancement of science; and therefore he could not advocate the doing away of medals or their restriction.

The Astronomer Royal felt that the society had existed for a great many years to promote the progress of astronomy, and that the award of the medal was one of the fundamental conditions of the constitution of the Society. They ought therefore to hesitate very much before tampering with it. He had not felt inclined to answer any of the arguments that had been put forward against the medal, because he had listened in vain for any reason which specially concerned the award. They had heard only vague generalities about the doings of other societies and about prizes given in schools; but this Society was not a debating Society, but one concerned with the science of Astronomy, and they ought to confine their remarks to cases that had arisen in it. He had not heard any statement that any award of the Society had done harm; on the contrary, if they looked over the list of medals they would find that the Society had shown recognition of very valuable work, and its sympathy with the workers, and that that sympathy did great good in promoting the progress of Astronomy. Whether they awarded a medal or not, they could not avoid making invidious distinctions. For instance, they had to decide what papers should be published in the Society's Memoirs. That might be considered invidious; but would Mr. Ranyard or Capt. Noble propose that no paper written by a Member of the Council should receive the honour of being published in the Memoirs?

Capt. Noble. I should not propose anything so absurd.

The Astronomer Royal said they selected a particular piece of work as worthy of recognition, and marked that by the award of the medal, and he regarded that as an essential part of the means they took for promoting the progress of science.

Dr. Huggins. I would like to show in two words the spirit in which the intelligence of the award of the medal had been received by Dr. Auwers, who had written to say that the award had filled him "with great joy." For thirty years the ambition of his life had been to devote his utmost energies to the advancement of Astronomy, his chosen science from early youth, but at times he had fallen under some feelings of doubt as to how far he had succeeded in realizing the idea he had set before himself. This award made to him by so distinguished a body as the Royal Astronomical Society had given him a feeling of satisfaction as to the past, of pleasure in the present, and of renewed hope and energy for the future. He accepted the medal with the greater pleasure as it was the recognition by Englishmen of his work on English Astronomy.

The Rev. S. Kinns wished to confirm what had been said by an incident of some years ago when the medal was presented to Mr. Goldsmith for having discovered 12 planets between Mars and Jupiter, the largest number found by any one man. A letter was afterwards received from the late Emperor of the French, expressing the very great pleasure the award had given him, and that incident would show how high the award was appreciated in foreign countries; and that it was not only an inducement to foreign astronomers to make these wonderful discoveries but a bond of union between English and foreign astronomers.

The President hoped that the motion now before the Meeting would not be carried, on the ground that it might tend to keep some of the most illustrious of the Society's Members from serving on the Council, because their friends would feel a delicacy in asking them to serve.

Mr. Ranyard. In regard to Mr. Freeman's remark that this motion had been brought on year after year, and ought now to be abandoned, he would remind them that all great questions must be discussed. [Laughter.] Persons opposed to it did not, of course, like it proposed again, because they were not able to produce arguments against it; but that was not the way to advance science. He had a right by the Bye-laws to bring this question on every year, and he must follow his conscience in what he considered right. He believed his ideas were making progress and he regarded Capt. Noble's motion as a half-measure towards his own. With regard to the President's remark that the best men would not serve,—

The President. I said we should scarcely like to ask them to serve.

Mr. Ranyard thought that no one would stand out, and it did not prevent the best men being asked or serving in other societies. It was acknowledged that good workers did not work for the sake of the medal, but because they liked their work. No worker, merely because he did not get a medal, would cease to work for the Society.

The Members then divided, when there were 23 for and 23 against. The President gave his casting-vote against the motion.

The President appointed Messrs. Maxwell Hall, Lewis, and Davis as Scrutineers of the Ballot, and the Meeting proceeded to the election of Council and Officers for the ensuing year. The following was the result of the Ballot:—

President: W. H. M. Christie. *Vice-Presidents*: J. C. Adams, Arthur Cayley, J. W. L. Glaisher, and E. J. Stone. *Treasurer*: A. A. Common. *Secretaries*: E. B. Knobel and Lieut.-Col. G. L. Tupman. *Foreign Secretary*: William Huggins. *Council*: Capt. W. de W. Abney, Hon. Sir James Cockle, Warren de la Rue, A. M. W. Downing, Edwin Dunkin, N. E. Green, Geo. Knott, E. W. Maunder, Capt. William Noble, Rev. S. J. Perry, Lieut.-Gen. J. F. Tennant, and H. H. Turner.

The Rev. John Mackenzie Bacon, Coldash, Newbury; *John Henry Brown*, 6 Cambridge Road, Brighton; *The Rev. W. R. Waugh*, The Manse, Portland, Dorset; were duly elected Fellows of the Society.

The following gentlemen were proposed for election as Fellows of the Society:—

J. E. Drower, May Trees, Endlesham Road, Balham. (Proposed by A. A. Common.)

George James Newbegin, Thorp St. Andrew, Norwich. (Proposed by Philip E. Sewell.)

Capt. George William Read, Broad Street, Penrhyn, Cornwall. (Proposed by Capt. H. Toynbee.)

Meeting of the Astronomical Society of France.

1888, February, 1.

M. C. Flammarion, *President*, in the Chair.

M. **Flammarion** described his visit to the Nice Observatory, and expressed his admiration at what he had seen there. In the great 30-inch equatoreal, the Orion nebula is marvellous, stars of the 16th magnitude seem bright; double stars from 0″·1 to 0″·3 apart are discernible. M. Flammarion observed the lunar eclipse of Jan. 28 at Nice. The Moon had a light copper tint during totality, and all details remained easily visible. The Observatory is 375 metres above the level of the Mediterranean. In the finder of the great equatoreal the Earth's shadow was fringed with a transparent border about 2′ in breadth.

MM. Henry and Trouvelot made some remarks on the eclipse, and the remarkable contrast with that of Oct. 1884, in which the Moon was scarcely visible during totality. M. Detaille said he had been struck by the very fine colour of the Moon; the shadow of the Earth, although ill-defined on the edge, was quite circular.

MM. Henry exhibited a photograph of the Pleiades taken with their 34 centimetre instrument, the time of exposure being four hours. The negative showed stars down to the 17th magnitude. Much new nebulous matter is discovered in this photograph. One of the bright stars is enveloped in a dense nebula hitherto unseen. Several singular long thin streaks of nebulous matter extend in some cases from star to star to a considerable length.

M. Bertaux, geographical editor, presented the Society with a new map of the Moon by M. C. Gaudibert. This map has been constructed from M. Gaudibert's observations and revisals; it has been drawn by M. Fenet, and reproduced by heliography. The diameter of the disk is 64 centimetres.

Meeting of the Royal Meteorological Society.

Wednesday, February 15, 1888.

Dr. Marcet, F.R.S., *President*, in the Chair.

Mr. T. S. **Ainge**, Mr. J. C. Bell, F.R.G.S., Mr. C. A. Markham, Surgeon-Major S. Smith, Mr. J. T. Tibbles, and Dr. J. Walther, were elected Fellows of the Society.

The following papers were read:—

1. "Electrical and Meteorological Observations on the Peak of Teneriffe," by the Hon. Ralph Abercromby. The author made a trip to the island of Teneriffe in October 1887, for the purpose of making electrical and meteorological observations. The results may be summarized as follows:—The electrical condition of the Peak of Teneriffe was found to be the same as in every other part of the world. The potential was moderately positive from

100 to 150 volts, at 5 ft. 5 in. from the ground, even at considerable altitudes; but the tension rose to 549 volts on the summit of the Peak (12,200 feet), and to 247 volts on the top of the rock of Gayga (7100 feet). A large number of halos were seen associated with local showers and cloud masses. The necessary ice-dust appeared to be formed by rising currents. The shadow of the Peak was seen projected against the sky at sunset. The idea of a south-west current flowing directly over the north-east trade was found to be erroneous. There was always a regular vertical succession of air-currents in intermediate directions, at different levels from the surface upwards, so that the air was always circulating on a complicated screw system.

2. "Rainfall of South Africa, 1842–1886," by Mr. W. B. Tripp, M.Inst.C.E. The author gives the rainfall statistics for all stations in South Africa, which possess records of ten complete years and upwards. He remarks upon the chronological succession of wet and dry years, and the consecutive years above and below the mean, describes the seasonal distribution of monthly maxima, and the extent over which monthly rains prevail, and concludes by comparing the curves of rainfall with those of sun-spot activity.

3. "Some Methods of Cloud Measurements," by Mr. Nils Ekholm. As exact cloud measurements afford almost the only easily available means of determining motions in the upper regions of the atmosphere, the author describes some methods which seem to him likely to give the best results, and gives details of the methods adopted at the Swedish Polar Station, Cap Thorsden, in Spitzbergen, and at the Upsala Observatory, for determining direction and angular velocity, and for making direct measurements of the height and absolute motions of the clouds.

Meteor Notes.

The recent severe weather has almost entirely prevented lengthy meteoric observations. Apart from this, the season is one during which few meteors are visible, and the present year apparently forms no exception, for on Feb. 13, during a watch of 5 hours, Mr. D. Booth at Leeds observed only 5 meteors, while on the 16th only one was recorded during 2 hours.

The Geminids appear to have exhibited unusual activity at their last return. Reports from various places have been printed in scientific journals, but the observers do not give very precise details. In 'L'Astronomie' for Feb. 1888, it is stated that "on Dec. 12, 1887, between $4^h\ 30^m$ and $5^h\ 30^m$ A.M., an observer at Meximieux (Ain) saw a remarkable shower of falling stars. Without doubt they belong to the swarm which is manifested during the period Dec. 6–13, and which is generally rich in meteors, but in the present year they appear to have returned with exceptional intensity. At 5 A.M. a shooting-star, brighter than all the others, lit up the sky and left a luminous train. It appeared in the west, at an altitude of 45°, and was directed horizontally to the north. This shower of falling stars was also observed at Beaulieu (Alpes-Maritimes)."

Mr. A. F. Shakespear, writing from Lüttichaustrasse, Dresden, to the 'English Mechanic,' says "numerous bright meteors were observed in and near this town on the nights of the 12 and 13 of December. At $8^h 35^m$, on the former date, one fell which illuminated the city and neighbourhood for some seconds, as if by electric light. It crossed the zenith, passing from east to west, and leaving behind it a long train of sparks like a rocket. Some smaller ones fell towards 10 o'clock, the direction being the same."

Bright Meteors.—On Dec. 4, 1887, at $9^h 55^m$, M. A. Lonvist at Melun, France, observed a very slow bolide falling almost vertically in the east, below Gemini, and a little inclined towards the S. It was of a beautiful bluish-white colour, and left a coruscating train. It endured 3 seconds. On Jan. 8, 1888, at about 4 P.M., a magnificent meteor, equal in size to the full moon, was observed at Porsgrund in the S.E. of Norway. It moved rapidly in an easterly direction towards Taurus. The fireball occupied 2 secs. in its transit; there was no detonation. On Feb. 12, 1888, Mr. T. P. Teale, writing from Leeds, says:—"On my way to Headingly Church this evening, within a few seconds of $6^h 30^m$, and looking due north, I saw a large meteor. It took an almost horizontal course from east to west, about 20° above the horizon, and seemed to last 3 secs., passing through an arc of about 20°. It disappeared, or at least became faintly visible, two or three times, as if passing behind thin layers of mist. It looked much like a rocket, and did not, as far as I could see, leave a luminous track, nor did it end in coloured sparks."

Fall of a Meteorite in Russia.—A large aërolitic stone descended at Krasnoslobodsk, in the Government of Pensa, on Sept. 4, 1887. The peasants in the fields were frightened at the formidable explosion, which sounded like a clap of thunder. They, however, recovered from their alarm, and broke the stone into pieces, to preserve the fragments as talismans. A piece of this meteorite, weighing about 4 pounds, upon being carefully analyzed, was found to contain crystals, which have all the chemical properties of diamonds. 'Nature' remarks that a meteoric iron from West Australia was some time ago discovered to contain similar corpuscles, small but well-defined crystals of graphitic carbon, having forms often presented by the diamond, being amongst its constituents.

Orbits of Meteors.—Mr. O. C. Wendell, of Harvard College Observatory, has computed the following orbits ('Sidereal Messenger,' Jan., p. 37), for the seven well-defined radiants observed at Bristol in September 1887, and published in the 'Observatory' for November last, p. 384:—

Current number.	Long. of Perihelion.	Long. of node.	Inclination.	Perihelion distance.
	°	°	°	′
1	106·9	173·3	5·7	0·300
2	87·7	175·2	67·4	0·478
3	24·6	174·3	85·6	0·932
4	94·7	175·2	127·3	0·418
5	52·2	173·3	178·5	0·758
6	43·4	177·2	51·0	0·846
7	49·5	173·3	69·5	0·778

Observations of Meteors in Hungary.—Dr. Josef v. Bártfay has just published ('Mittheilungen vom Observatorium des kön. Polytechnicums,' 1887) the results of 16 years' observations at O'Gyalla, Budapest, and other stations. He gives a list of 64 stationary meteors, also a table of the orbits of 152 meteor-streams. The paths of 222 meteors, observed chiefly in July and August 1886, are also included in this work, together with 6 radiant-points derived from them. W. F. DENNING.

Bristol, 1888, Feb. 18.

Selenographical Notes.

RILL FROM ROSSE TO BOHNENBERGER *f*.—On the evening of 19 Dec. 1887, when the W. long. of the morning terminator was about 34° 30′, the rill marked ϕ in Neison's Map xxii. was easily traced from the extremity of the shadow of the crater *f* north of Bohnenberger up to the west side of Rosse, in the form of an undulating black line, crossing the prominent ridge east of Bohnenberger very obliquely. Schmidt omits this object from his chart, and Mädler shows only the northern section of it, though it seems to be obvious enough at this stage of illumination in good observing weather, even with low powers. But for its sharpness it might well be set down as a low ridge, as its flanks appear to be so steep that they are scarcely visible except here and there. It is worth remarking that the brilliant ray, so conspicuous under a high sun, which traverses the Mare Nectaris in a north-westerly direction from Rosse, covers the site of this rill, affording another example of the association of these features.

MARIUS.—Since describing the various craterlets which have been seen from time to time on the wall of this formation, I have received a communication from Mr. W. Goodacre, in which he records the observation of the circular pit on the outer slope of the eastern border, first seen by Mr. Davis of Theale. He saw this object as a crater at 10^h on Dec. 26, 1887, with a power of 300, on a 12-in. reflector, just under the crest of the wall. I was observing Marius on the same evening, but failed to see it, as the illumination was probably not sufficiently advanced at the time.

Landsberg.—At 8^h on January 23, I noted a well-marked semicircular notch on the west boundary of the interior shadow of this ring-plane, pointing to the existence of a good-sized crater on the summit of the west wall. E. long. of morning terminator about 32°. No such object is shown in the maps. A curved valley runs up the western *glacis* to the notch, and then, trending northwards, again joins the plain. Neison (Map xiii.) shows a curious formation extending from south-east side of Landsberg to Landsberg C. No trace of this was seen, the interval being apparently entirely void of detail—Landsberg *g*, figured as a dark ring full of shadow, contrasting with the other brilliant objects of the same type in its neighbourhood. A minute crater was seen on the north border of Landsberg C, and a similar feature suspected in the same position on Landsberg B. The craters *c*, *e*, and F stand on the edge of a ridge, which at this stage casts a prominent shadow. The nimbus of light which under a high sun encircles A, B, *c*, *d*, *e*, was only faintly traceable in the case of *c*.

Euclides.—A very bright little crater was observed near this brilliant object on Jan. 23, which, strange to say, is not shown by Mädler, Neison, or Schmidt, who draw, however, two or three small mounds in the neighbourhood. It is situated about one and a half diameters of Euclides W.S.W. of this formation. At 8^h 40^m it was full of shadow, and so conspicuous with a power of 284 on my 8½ Calver-reflector, that I cannot imagine the possibility of its being overlooked under similar conditions.

Hercules.—Under a high angle of illumination, four or five light spots, arranged in the form of a horseshoe, are visible north of the large crater D, on the floor of this formation. Two of these objects were seen as distinct craters on Jan. 23, by Mr. W. H. Maw, of Kensington, with powers of 120 and 180 on a 6-in. achromatic by Cooke.

Kempston, Beds, Feb. 16, 1888. Thomas Gwyn Elger.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The 'Nautical Almanac.'

Gentlemen,—

It is with the most sincere gratification that I have read General Tennant's article on the contents and arrangement of the 'Nautical Almanac' ('Observatory,' No. 133). The office in Verulam Buildings supplies so admirable an illustration of the principle of Inertia, that if any change is to be initiated at all in the work for whose production it exists, such change can and will only be effected under the strongest external pressure. Hence as having myself, for a long time, in my own feeble way, attempted to indicate certain alterations which I regard as improvements, I welcome the advent among reformers of a man whose intimate acquaintance with astronomy in (*inter alia*) its geodesical relations enables him to speak so thoroughly *ex cathedrâ* as does that of your eminent contributor.

One anomaly, at which I have been hammering for years (R.A.S. 'Monthly Notices,' Vol. xxxii. p. 211, *et alibi*), I mean the arbitrary and exceptional method of measuring occultation angles round the Moon's limb, has, I am gratified to find, been swept away in the issue of the 'Almanac' for 1891. But this is a comparatively trivial matter, and, as General Tennant points out, there is still much room for improvement. Speaking broadly, the 'Nautical Almanac' in its existing form is compiled in the interest, (1) of the navigator, and (2) in that of the official astronomer; the large and increasing number of amateur astronomers, many of them doing work of high and enduring value, being, so far as its contents go, very nearly left out in the cold altogether. Like General Tennant, I quite fail to see why page after page of, more or less erroneous, predictions of the phenomena of Jupiter's Satellites should appear, while those of Mars, Saturn, Uranus, and Neptune are wholly ignored. As it is, your own excellent little "Companion to the Observatory" is an absolutely indispensable adjunct to the National Ephemeris; and though I for one am really grateful to you for this, yet it surely ought not to be so. Then, again, with reference to the list of Stars, it seems to me that an urgent want is a considerable addition to the number of close circumpolar ones. That four (!) and four only should appear in the list seems something like a mockery to those who, like myself, have often had to wait for hours for a highly necessary observation. Further, it is much to be desired that the relative dimensions of the Saturnian system should be revised from more recent and trustworthy data. To any one who doubts this I would simply suggest that he should make a careful outline of Saturn and his rings from the data supplied on pp. vi, 284, and 464 of the 'Nautical Almanac' for the present year, and compare it with the actual aspect of the planet as viewed with adequate power in the telescope; he will, I think, be instantly and forcibly struck with the fact that either the 'Nautical Almanac' dimensions of the rings are too large, or those of the ball notably too small (*vide* Vol. iii. of the 'Journal of the Liverpool Astronomical Society,' plate facing p. 116, as an illustration of this). In all departments having reference to matters of detail concerning the planets, the American 'Nautical Almanac' is decidedly in advance of the British.

In what I have said I have confined myself to two or three points in which the almanac appears susceptible of improvement, as far as the wants of the ordinary amateur astronomer are concerned. Those of the Gravitational and Geodetical Astronomer need no better exponent than General Tennant.

Forest Lodge, Maresfield,
Uckfield, 1888, Feb. 13.

Faithfully yours,
William Noble.

Gentlemen,—

I notice with pleasure General Tennant's article upon the present state of the 'Nautical Almanac,' in which, I think, many improvements and changes might now be introduced with advantage.

The fact, however, that the control of this ephemeris belongs to the Admiralty, and that before any change is made committees must meet and reports be considered, will no doubt cause much delay before we shall have the use of General Tennant's ideal 'Nautical Almanac.'

As regards the Sun's longitude, latitude, and coordinates, I can only echo Gen. Tennant's words and express a hope that these quantities may be referred to the mean equinox of the beginning of the year and not, as at present, to the true equinox of date. A like remark applies to the heliocentric places of the planets. The tables of the Sun and planets give the coordinates referred to the mean equinox. To refer these to the true equinox of date, a certain correction must be applied, and when these coordinates are used by calculators they must again apply this correction with converse sign. To refer, then, to the mean equinox is to save trouble all round.

I disagree with General Tennant when he says lunar distances are still necessary for seamen. He adds, however, that the number of vessels where they are never taken is very great and is probably increasing. If the longitude be determined by a lunar distance it cannot be relied on. Prof. Boss states that with a well-divided sextant the probable error of reading is 14″ (the instrument being cut to 10′); to this must be added the error of eccentricity, which varies with the reading, but which may amount to 30″, the errors due to the prismatic forms of the shades and of the horizon glasses, and the error caused by not bringing the star in contact with the nearest point of the Moon's limb; is it, then, surprising that we get a longitude 15′ or 30′ different from the truth?

As regards moon culminating stars, the editor's remarks show that it will be better to retain them. Culminators have been used for planets with advantage where great accuracy has been aimed at, *e.g.* for Sappho in 1882. It is, I think, in respect to minor planets, when a certain amount of work is at our disposal, better to expend it over a large number, giving less extended ephemerides, than over a small number, giving ephemerides to a far larger extent than is generally required. This would, to a certain extent, lighten the work in this direction of the 'Berliner Jahrbuch.'

To increase the number of standard stars would doubtless be an advantage, but the difficulty of continuous observation of them at

Greenwich would, as the editors remark, be a hindrance to the regular work there. Yet in temporary observatories, such as are often erected in scientific expeditions, stars whose positions are less accurately determined than those of the present standard star list might be used in many instances. If the places of such stars were introduced into the 'Nautical Almanac,' in order to show that their positions have not the degree of accuracy of the present standard star list, it might be advisable to omit the last figure in the positions given for "Apparent Places of Stars at the upper Transit at Greenwich"; the error under such circumstances would not exceed $0^s{\cdot}05$ and $0''{\cdot}5$ in R.A. and N.P.D. respectively. Or the third decimal of the seconds of Right Ascension and the second decimal of the seconds of Declination in the list of "Mean Places of Stars" might be omitted.

In the arrangement the only point to which I would draw attention is, that as nautical men confine their attention almost exclusively to pages I & II of each month, it might be preferable to keep these two pages separate instead of putting them in with positions for transit at Greenwich as suggested by General Tennant.

I should like to see the adopted masses and the elements of the orbits of the major planets given; but if everybody's suggestion of improvement consisted in the addition of a few items, the size of the volume would soon be doubled.

A word in conclusion as to the abominable piracy that is carried on in respect to this publication. The editors notice a similar act in regard to the 'American Ephemeris.' The 'Nautical Almanac' is an official work, published at the expense of the nation, and appears three or four years in advance of the time for which it is required. Ample time is thus given to pirate its contents and sell them at even one-fifth of the present low price of the 'Nautical Almanac,' all the expense of calculating the latter being borne by the country.

Faithfully yours,

Robert Bryant.

Saturn.

Gentlemen,—

The weather in this part of England has, till quite recently, been more or less unfavourable for observation of the delicate details of the planet, but on Feb. 10, 11, and 12, definition being very good, the following notes were made, which were substantially similar on each occasion as regards the features referred to.

Ring A.—An evident narrow light stripe was visible on the west ansa at a distance from the inner edge about equal to one-third the total width of the ring. On either side of the stripe the ring appeared to be equally dark on 12th February. It was not seen on the east ansa.

Cassini Division.—On each night the outer edge was less sharply defined than the inner. On 12th February, $8^h\ 30^m$ to $9^h\ 10^m$, dusky notches or projections from the outer edge (on the east ansa), extending some distance on the surface of A, were noted, very similar to those recorded by MM. Stroobant and Stuyvaert last spring. This division could be much more easily traced on the south side of the ring than on the north.

Ring B.—Appeared gradually to increase in brightness from the inner to the outer edge, no trace of Dr. Terby's distinct zones being apparent. On 12th February, in the east ansa, the outer rim was bordered by a narrow well-defined brilliant band scarcely traceable in the opposite ansa. Last year this feature was generally visible in both ansæ.

Ring C.—On each night this ring was very much lighter in the west ansa than on any occasion when it was observed last year, and perfectly regular in form and tone, except a small portion of it adjoining the ball, *s. p.*, which was very dark. In the east ansa, however, it was noted as very dark, difficult to trace in places, and evidently in the same abnormal condition as was often the case with the same ring in the west ansa last February, as recorded by Dr. Terby and myself.

The Belts have materially changed in arrangement. The dark equatoreal band was sharply defined on the north side on the 12th inst., and was succeeded on the south by a much fainter band (with a narrow intervening zone devoid of detail) and then by an apparently uniformly light grey region extending to the dark bluish-grey polar cap. The whole ball, with the exception of the region near the pole, was rosy.

The shadow of ball on rings was noted as bounded by a concave line, interrupted by a slight notch at the Cassini division. Powers of 340 and 420 on an 8½-in. Calver-reflector were used on each evening.

Yours faithfully,

Kempston, Beds, 1888, Feb. 16. THOMAS GWYN ELGER.

The Solar Eclipse of B.C. 49.

GENTLEMEN,—

Reference is sometimes made to an eclipse of the Sun which has been said to have taken place when Julius Cæsar made his celebrated passage of the Rubicon. Now I believe the only ancient authority for an eclipse having been seen in the *year* of that event is Dion Cassius, who mentions it very indefinitely whilst speaking of Pompey's passing over to Dyrrachium (which was undoubtedly in the autumn of B.C. 49), when he says that several prodigies occurred at the time to himself and others, in that year or a little before, which affected the whole state:—*ἐκείνῳ μὲν δὴ ταῦτα τὰ τέρατα ἐγένετο· συνεβεβήκει δὲ καὶ πάσῃ τῇ πόλει τούτῳ τε τῷ ἔτει καὶ ὀλίγον ἔμπροσθεν ἕτερα.*

One of these was that *ὅ τε ἥλιος σύμπας ἐξέλιπε*, so that he gives us very little indication of the exact time of this eclipse, if eclipse it were. But it is quite certain that it did not take place when Cæsar was crossing the Rubicon, which was very early in the year B.C. 49, or perhaps (there was, as is well known, great confusion in the Roman calendar about that time) towards the end of the year B.C. 50. Pétau, indeed, under an erroneous idea of the chronology of that period, thought that it was the eclipse of the 7th of March, B.C. 51, which was annular in some places. It is more likely that Dion is referring to the eclipse (also annular) of the 9th of August, B.C. 49. This would certainly be not very far from the time when Pompey crossed the Adriatic, in connection with which (as I said before) mention is made of an eclipse by the historian, though in such an indefinite way that it does not help us much in fixing the terrestrial events of the year.

Yours faithfully,

Blackheath, 1888, Feb. 3. W. T. LYNN.

The Visibility of the Unilluminated Part of Venus.

GENTLEMEN,—

In a paper contributed to the 'Astronomische Nachrichten' (No. 2818), Herr J. Lamp speaks of having seen last October (about a month after the planet was in inferior conjunction) that peculiar faint ashy light on the dark part of Venus which has been frequently noticed at certain times, and has led to much discussion. Prof. Zenger has a paper on the subject in the 'Monthly Notices' for April 1883 (Vol. xliii. p. 331), in which he calls attention to the fact that the first account of a perception of a similar kind is that of Riccioli on the 9th of January, 1643, and that the appearance had been subsequently noticed by Kirch in the eighteenth, and by Harding and Schröter early in the present century. Riccioli gives a drawing as well as a description (Almagestum Novum, Vol. i. p. 484), by which it is evident that he saw the whole circle of Venus surrounding this ashy (greenish-blue it appeared to him) light on the dark side of the planet. Some have suggested that this faint light may be due to a cause similar to that which produces the visibility of the unilluminated part of the Moon when near conjunction; and this view is even alluded to by Prof. Zenger with apparent approval in the last paragraph (of the "Addition") of his paper in the 'Monthly Notices.' But, as Herr J. Lamp remarks, it seems very difficult to assent to it. The ashy light of the Moon when near conjunction is, as is well known, due to re-reflected earth-light. But the existence of a satellite capable of illuminating to any perceptible extent the dark part of Venus cannot now be admitted; and it is scarcely conceivable that the reflected light of a distant planet could produce any such effect. He therefore concludes that it is due to some phosphorescence or electrical action in the atmosphere of Venus.

It is remarkable, as Prof. Newcomb points out in his 'Popular Astronomy' (2nd Edition, p. 304), that this appearance (which is only seen occasionally) does not appear to have been noticed excepting "in the daytime or during bright twilight, rarely or never after dark." From this circumstance he is disposed to infer that it is "due to some unexplained optical illusion," since if real it should be most conspicuous after dark.

When I wrote my letter (dated 1883, May 2) in the 'Observatory' for June 1883, I of course had not seen Prof. Zenger's paper in the 'Monthly Notices'; but I ought to have known of the early observations of this phenomenon to which he refers, instead of supposing that it was first seen by Mr. Prince at Uckfield in 1863. His observations were made in the middle of the day; of those made in twilight Schröter's, in the month of February 1806, were, I believe, at a time in the evening when it was fainter than during any similar observations. It is evidently desirable that all who make similar observations should record their exact time.

Yours faithfully,
W. T. Lynn.

Blackheath, 1887, Dec. 21.

The Star of Bethlehem.

Gentlemen,—

The writer of the leading article in 'Nature' for Dec. 22, 1887, after three columns of considerable value and interest, has apparently indulged in a *jeu d'esprit* wherewith to tickle the imaginations of those who have followed him so far. As it happens, his guess that Venus at its greatest morning brilliancy was possibly what the Magi saw, seems to be quite without foundation.

The best authorities, with good reason, place the birth of Jesus Christ in December B.C. 5, or else in January or February B.C. 4, not earlier nor later. Let us assume the date Dec. 25, B.C. 5. From noon of this day to noon of Jan. 1, 1850, is exactly 676,802 days. The mean heliocentric motion of Venus exceeds that of the Earth by 2219″·4771 in one mean solar day (Faye, 'Astronomie,' 2^{me} partie, p. 407). The excess in 676,802 days is therefore 1502146540″·2342, which is equivalent to $1159\times360^\circ+22^\circ\ 55'\ 40''$, neglecting decimals of 1″. Now on Jan. 1, 1850, at noon, *Paris* (Faye, *loc. cit.*), the mean longitude of Venus exceeded that of the Earth by 144° 46′ 31″. Hence, on Dec. 25, B.C. 5, the excess of Venus' mean longitude over the Earth's was 121° 50′ 51″; but when at its position of greatest morning brilliancy the excess is 22° 20′ 55″ (with Leverrier's value of the mean distance of Venus). Thus, at the assumed date of the first Christmas, Venus was 99° 29′ 56″, heliocentrically, beyond the position of greatest morning brilliancy (as seen from the Earth), which *in time* would amount to 161·387 days nearly, or about 5¼ months after.

But a more serious objection to the assumption that Venus was mistaken by the Magi for a new star arises from the fact that the morning and evening stars, long known by the names *Phosphorus* and *Hesperus*, were identified as different aspects of the same planet so far back as B.C. 540 (Sir G. C. Lewis, 'Astronomy of the Ancients,' p. 62, note 234). It is not probable that the Magi would have been ignorant of this discovery.

Faithfully yours,
A. Freeman.

Murston Rectory, Kent,
1888, Jan. 18.

The Total Eclipse of the Moon.

Further intelligence allows us to add the following items to the table given on p. 131 of our last number:—

Place.	Instr.	Observer.	Number of Obs.		Remarks.
	in.		Disap.	Reap.	
Edinburgh...	3½	Heath	3	1	
Geneva	10	Kammermann ...	6	17	Some doubtful.
Markree......	...	A. Marth	12	4	Very fine. Also 1 disap. and 5 reap. doubtful.
Sunderland.	...	T. W. Backhouse	1	...	Generally fine. No systematic observations undertaken.

The Moon was at most places noted to be of the usual deep-red colour. M. Gautier, at Geneva, remarks that before totality a faint blue illumination was noticeable on the dark limb. The eclipse was watched under favourable conditions by Mr. G. C. Thompson at Penarth, near Cardiff, who mentions a greenish colour of the limb during totality. Both these effects are possibly the effect of contrast with the ruddier centre. Dr. Copeland describes the tint as cherry-coloured, and says that it cannot be due to light from the solar prominences, because no trace of the C line could be detected in the spectroscope; and this result is confirmed by the observations of Prof. Piazzi Smyth, who has kindly drawn our attention to a theory of Prof. Filopanti, of Bologna, as regards this colour. The following is a translation of a note by him in 'Il Resto del Carlino' for Feb. 4:—

"In my former long article written for the public I expressed my suspicion, or almost conviction, that the red colour of the eclipsed Moon arose not only from the solar light refracted in its passage through the Earth's atmosphere, but also from some physical or chemical change peculiar to the Moon's surface. It may be well to add, for the benefit of scientists, some special reasons for my opinion.

"1. Many opaque bodies, perhaps all, after having been exposed for a long time to a strong light, and having been, so to speak, saturated with it, keep for some time a sort of phosphorescence.

"This phenomenon is especially seen in sulphate of barium when made into a paste and exposed to the Sun. It is called, or used to be called, 'pietra fosforica di Bologna,' as the mineral was found abundantly at Mount Paderno, near Bologna.

"Now the Moon remains exposed to the heating of the Sun's rays for a period of about 15 consecutive days without the protection of a sensible atmosphere. While the middle of the illuminated hemisphere must be burning hot, even more so than the sands of the Sahara, the circumference is cold, and the dark hemisphere is extremely cold. The passage from the extreme of heat to the extreme of cold under ordinary circumstances occupies about 15 days, but in the eclipse of the Moon it takes a single hour ora little more.

"2. The bright reddish colour of the eclipsed Moon has neither the middle tint nor the blending and gradations of the twilight proceeding from the larger or smaller quantities of aqueous vapour contained in the different strata of the Earth's atmosphere.

"3. In eclipses the whitish illumination occupies the circumference, the reddish occupies the centre, which assumes the appearance of a sick and troubled Moon.

"4. The pale and grey illumination, caused probably by refraction alone, follows immediately the beginning and extension of the obscuration; but the red colour, caused probably not by refraction only but also by a physical change in the surface of the lunar rocks, does not show itself distinctly until several minutes later, notwithstanding that the light from the Earth to the Moon arrived in a little more than a second of time."

OBSERVATORIES.

Wolsingham Observatory.—During last year sweeping for red stars and stars with remarkable spectra has been continued. Spectra of 126 objects have been published in the A. N., Nos. 2788, 2825. Of these 86 were found in the sweeps; the rest are from Birmingham &c.; 15 are IV. type. Three new long-period variables have been discovered in the year. The usual variable-star observations have been made and forwarded to Harvard College. A fine 4·8-in. equatoreal-refractor, with clock, circles, micrometer, &c., all by Troughton and Simms, has been presented by Canon Slatter; and several donations towards paying off the debt on the 17-in. equatoreal have been received. Numerous requests to confirm observations were received and attended to during the year. The new edition of Birmingham's Red-Star Catalogue will be ready for the printer in a few weeks. T. E. Espin.

Rousdon Observatory, Lyme Regis.—Mr. C. E. Peek has a good record to show for the year 1887. Observations were made on 165 nights in the year, with the result that 20 long-period variables have been examined with the 6·4-in. refractor on the average 14 times apiece; and transit observations taken for time, which is supplied to the district, on all available nights.

Dunsink Observatory.—The "Sixth Part" of the publications of this observatory contains the observations made in the years 1881-1885 with the transit-circle on 1012 stars of the Southern Durchmusterung, pointed out by Prof. Schönfeld as requiring reobservation. This work seems to have been done thoroughly well by Dr. Dreyer, and Mr. Rambaut who succeeded him in 1882.

Considering the faintness of the stars and their low culmination, the probable errors of $+0^{s}\cdot065$ for a single observation of R.A., and $\pm 0''\cdot864$ for N.P.D. reflect credit on the observers. There is a curious remark in the introduction to the effect that cleaning the transit-wires from fungus altered all their intervals. From the evidence given, it would appear that the middle wire only was displaced; possibly this may have been broken and replaced, and the fact by some oversight not recorded; or there may have been an error in one of the reductions of its place. We can scarcely believe that the disturbance mentioned can have occurred.

U.S. NAVAL OBSERVATORY.—From the Annual Report, dated 1887 October 5, we gather that Capt. R. L. Phythian has replaced Commander Brown as Superintendent of this observatory. The great equatoreal has been kept at work on the planets, especially Saturn, and on double stars; and the 9·6-inch equatoreal devoted to comets, occultations, and the revision of anonymous stars in Yarnall's Catalogue. Of these last, over 2000 have been examined, and 842 corrections found necessary. The meridian observations of Sun, Moon, planets, and standard stars have been continued, but for want of computers have not been reduced after 1885. The care of chronometers and time service has occupied a considerable portion of the attention of the staff.

The volume of observations made in the year 1883 has been published. With the transit-circle, the Sun, Moon, and major planets were observed, and their diameters and places compared with the tables as in former years. The stellar catalogue for the year gives the places of 638 miscellaneous stars; and a large number of observations were also made on the N.P.D.'s of standard stars. Right ascensions of the planets have also been observed with the transit instrument. The 26-in. equatoreal has been used for observations of satellites as before, and the 9·6-inch equatoreal for minor planets. Two important memoirs by Prof. Asaph Hall on the Satellites of Saturn and Stellar Parallax, which are printed as appendices to this volume, were circulated some time ago, and have already been noticed in the 'Observatory' (see No. 121, pp. 140 and 142).

HARVARD COLLEGE OBSERVATORY.—Part ii. of Vol. xiii. of the 'Annals' has been published, and gives the results, in a memoir of 150 pages quarto, of zone observations with the "Transit Wedge-Photometer" attached to the 15-inch equatoreal. This apparatus consists of a photometric wedge and a bar placed perpendicular to its length. If the wedge be set in the direction of motion of a star, with the thin end and bar on the right, the star after entering the field will first be occulted by the bar, and then disappear in the wedge at a point determined by its magnitude. Observations of the times of these two occurrences for a series of stars thus give their relative right ascensions and magnitudes, and the relative declinations can be estimated by noting the height on the bar at which the star is occulted. As there are various difficulties in the way of making these measures of magnitude absolute, Prof. Pickering has regarded them as simply differential, and corrected the measures on any night by comparing the deduced magnitudes of a number of standard stars with their magnitudes as determined by his meridian photometer. The object of the experiments was to compare the magnitudes of the fainter stars (mag. 9·0 to 9·5) in the Durchmusterung with those given by the meridian photometer; and accordingly the magnitudes of the stars in three zones, each of 10′ breadth, in declinations +1°, +50°, and +55° respectively, were determined by the above method (which is practically equivalent to their determination by the meridian photometer, but much more expeditious), and compared with the magnitudes given in the Durchmusterung. The following is an extract from his table of results, which includes also stars not fainter than the ninth magnitude directly observed with the meridian photometer:—

D.M.	Mer. Phot. Mag.		
Mag.	+1°	+50°	+55°
7·0	6·8	7·2	7·2
7·5	7·3	7·7	7·7
8·0	7·9	8·2	8·2
8·5	8·5	8·7	8·7
9·0	9·2	9·3	9·3
9·5	10·5	10·5	10·5

It will be seen that in the case of the brighter stars there is a discrepancy between the zones of greater and less declination, but the mean magnitude accords well with that of the D.M.; but for the fainter stars the discrepancy between the zones has disappeared, while the correction to D.M. magnitude has become a whole unit. The work has obviously been done very thoroughly, and the memoir is of considerable interest.

We have received Prof. Pickering's report for the year 1887. He gratefully acknowledges "three important accessions to the resources of the observatory" in the Draper, Boyden, and Paine funds; but is not so indiscreet as to express himself content. Indeed, if we understand him rightly, his needs seem to have been rather increased than otherwise by this accumulation of wealth. But he is prompt to follow up this imitation of a celebrated hero of romance in "asking for more" by such a satisfactory account of work done in the past year as must recommend his claims to the severest auditor. The Boyden fund, for instance (bequeathed for conducting astronomical observations at such a height as to be free, as far as possible, from the injurious effects due to the atmosphere), has been so energetically administered that the climates of seven or eight places in Colorado from 11,000 to 14,000 ft. above the sea-level have been tested; instruments specially devised for observing and recording the amount of cloud at night, the steadiness of the air, the clearness of the air, the transparency to violet rays, and the diffusion of stellar images, besides ordinary meteorological instruments, having been transported to each of these places in succession. Further visits to the Sandwich Islands, to Peru, and again to Colorado are contemplated.

The progress of the work in connection with the Henry Draper Memorial has previously been reported. The first research undertaken has been confined to photographing the spectra of the brighter stars north of −25°. The total number of stellar spectra photographed is 27,803, of which the greater portion have been measured, and the stars have been identified and their places computed for 1900. The preparation of a catalogue will shortly be undertaken. A second research will include the spectra of fainter stars; and 9880 of these have already been obtained—more than a quarter of the work. A third research is confined to the spectra of stars brighter than 3rd magnitude, which are carefully enlarged, and in some of which several hundred lines are visible. Mrs. Draper's continued liberality is also extending the work in other directions.

As regards the ordinary work of the Observatory, the meridian photometer has been kept at work on the zones of the Southern Durchmusterung; a Pritchard wedge, mounted on the east equatoreal and found satisfactory, is to be employed on zones of D.M. stars. The meridian work has been almost entirely stopped, to allow of the reduction and publication of accumulated results, with which considerable progress has been made.

LICK OBSERVATORY.—Work was commenced with the 36-in. refractor on Jan. 7. From the 'Sidereal Messenger' we learn that the dome could not be turned, as the non-congealing solution which will fill its "liquid seal" has not yet been provided, and everything had been frozen solid by a week of severe weather. It was consequently possible to observe an object only during the half-hour of its transit across the shutter-opening. The observers profess the greatest satisfaction with the views obtained of the Orion nebula and Saturn. New details were made out in the rings of Saturn which can be reconciled with previous observations made with smaller telescopes, but at the same time throw new light on them. The satellite of Neptune was very distinct, and Mr. Keeler is of opinion that another satellite would have been detected if only one fifth as bright as that known. Altogether the trial observations may be considered extremely satisfactory, and we hope for early intelligence of further success, for which the observers have our best wishes.

SYRACUSE OBSERVATORY.—A new observatory was "dedicated" on 1887 Nov. 18 to the memory of C. D. Holden, of Syracuse University. It is equipped with a 3-in. transit by Troughton and Simms, and an 8-inch equatoreal by Clark Bros. Prof. J. R. French has been appointed director.

MELBOURNE OBSERVATORY.—The report of this observatory for the year ending 1887 July 1 has been published. The ordinary work has proceeded on much the same lines as before, except that the surface of the mirrors of the great telescope has deteriorated so much that work on the fainter nebulæ has become impossible. The question of repolishing them is under consideration. The new transit-circle has settled down into a stable position, although the level error is still affected by wet weather. The 8-inch sun-pictures obtained with the photoheliograph are not so satisfactory as the 4-inch pictures previously taken. A similar result has been noticed at Greenwich, where it is intended to return to the smaller pictures in consequence. Early publication is promised of the results 1880–85, and the second general catalogue of stars is being copied for the printer. The sanction of the Government has been obtained for the purchase of an instrument which will enable the Observatory to take part in the stellar photographic scheme of the Paris Conference.

WINDSOR, NEW SOUTH WALES.—Mr. Tebbutt has published, in pamphlet form, a most interesting account of his observatory; from which we take a few extracts for the benefit of those of our readers who have not been fortunate enough to see a copy:—

"At the eastern extremity of the municipal town of Windsor lies the Peninsula Estate, a tract containing about 250 acres of the richest alluvial land. It is so called because it is nearly surrounded by the courses of the Hawkesbury River and its tributary the South Creek at their confluence. On a hill situated a little south-west of the middle of the estate, and whose summit is about fifty feet above the local mean tidal level, stands the residence of the author, with the two Observatories now to be described. Occasional astronomical observations, necessarily of a rough character, were made by him at his residence from 1854 to 1864, the instruments employed being only a sextant, an artificial horizon, a telescope of 1⅝ inches aperture, and a common but excellent eight-day clock with seconds pendulum. These were employed chiefly for the purpose of self-instruction and for providing gratuitous information of a popular character for the daily newspapers. To these instruments were added a refracting telescope by Jones, of 3¼ inches aperture and 48 inches focal length, in November 1861, and an excellent eight-day half-seconds box-chronometer by Parkinson and Frodsham, No. 1042, in April 1864. At the close of 1863 a small observatory was erected on the western side of his residence, the walls being constructed of pine and the roof covered with slates. The building comprised a transit-room, with two meridian piers and their corresponding openings in the walls and roof, and likewise a prime vertical room. The local mean time was determined by means of a transit instrument made expressly for the observatory by A. Tornaghi, of Sydney, and mounted in September 1864.

"On the bank of the South Creek, south of the Observatory, stands a meridian mark corresponding to the western or double pier before described. A stout iron-bark post is sunk perpendicularly into the ground to the depth of about five feet, its top being nearly level with the northern sloping surface of the bank. About six inches below the top a piece of white porcelain is let into the post, and over this is secured a plate of copper painted black, with a circular hole in it about a quarter of an inch in diameter. The porcelain is thus seen through the perforation from the Observatory as a small white spot on a black ground. This spot long served as a meridian and collimating mark for the transit-instrument, its centre from the most recent observations being 7″·7 east of the meridian.

"Notwithstanding the imperfections of the instrumental means, every precaution was taken in the work of observation so that the error of determination of the local mean time, from a set of transits on any evening, could hardly exceed half a second.

"A small octagonal tower rising from the 'centre of the Observatory, and covered by a conical revolving roof, accommodated the 3¼-inch refractor already referred to. It was furnished with two ring-micrometers by Tornaghi, and with this telescope all the extra meridian observations were made till the acquisition of a 4½-inch equatorial in June 1872. In 1874, a circular building of pine, twelve feet in diameter, was erected a few yards southeast of the Observatory already described, for the temporary reception of the latter instrument.

"The illuminating arrangement has always given complete satisfaction, but there being no means of exhibiting bright threads on a dark field, all faint objects were, until the close of 1881, observed by means of the ring-micrometers. It is well known to astronomers that it is only under certain conditions, which cannot always be secured, that differential measures with micrometers yield satisfactory results. Since 1881 the use of the ring, except in one particular instance, has been abandoned, and for the observation of faint objects a square bar-micrometer has been substituted.

"In 1879 a substantial observatory of brick was erected a few yards south-west of the old building. From the centre of a large room at its west end rises a pyramidal pier of brick and cement, which passes through the ground-floor and a floor above. To this pier the 4½-inch equatorial was transferred at the close of August 1879. The instrument has remained there down to the present time, and is protected by a galvanized-iron roof revolving on ten wheels on a circular plate faced with iron. The opening in this roof extends from horizon to horizon and is closed by six shutters, two of which open outwards and the others inwards. The apartment under the equatorial room serves as an office and computing room. On the south side of this apartment is the prime vertical chamber with its pier, and on the eastern side the meridian transit-room for time observations. The meridian room is furnished with an excellent transit-instrument made for the observatory by the Messrs. Cooke, and mounted on a substantial pier in September 1879.

"On the eastern side of the transit-room is a fire-proof apartment sometimes used as an office and computing room. It also contains the astronomical library, which is gradually being enriched by works of necessary importance to the practical astronomer.

"It now remains to speak of a very important addition to the instrumental equipment of the observatory. This consists of a fine equatorial refractor of 8 inches clear aperture, and 9 feet 7 inches focal length. It was constructed in 1882, by Grubb, of Dublin, and imported by the late Dr. Bone, of Castlemaine, Victoria. On the death of Dr. Bone the instrument was sold to the writer for the sum of £400, or about two thirds of the original cost. The telescope reached the observatory on May 20th last year, and on the 16th of the following month was permanently mounted in the circular building formerly accommodating the 4½-inch equatorial.

"The Observatory being now regarded as a fundamental point in the Australian system of longitudes, Professor Harkness, of Washington, has recently suggested the great importance of determining with all possible accuracy, by means of the electric telegraph, the difference of longitude between it and the Government Observatories at Sydney and Melbourne, two other well-determined points in Australia. At the request of that well-known astronomer this work has recently been carried out, and the results promise to be exceedingly satisfactory. In concluding this brief description of the observatories and their appliances it may be added that a considerable portion of the results represented in the accompanying list of papers has been already utilized, and the work has proved of sufficient importance to admit of Windsor being placed in the list of observatories in the British and American Nautical Almanacs, the 'Connaissance des Temps,' and the 'Berliner Astronomisches Jahrbuch.'"

PUBLICATIONS.

ASTRONOMY FOR AMATEURS *.—Mr. Oliver's book has been written to supply an important place in modern astronomical literature. It is intended for a class of readers which the prosperity of societies like the Liverpool Astronomical Society shows to be steadily increasing—men who mean honest hard work, even though it be in their so-called leisure moments. That the book is addressed chiefly to such men is made abundantly clear by sen-

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* 'Astronomy for Amateurs: a practical manual of telescopic research in all latitudes, adapted to the powers of moderate instruments.' Edited by John A. Westwood Oliver. (Longmans.) Small 8vo. Price 7*s.* 6*d.*

tences like the following:—"When a first-class night comes, the earnest observer will make the most of it, and will, of course, indefinitely postpone going to bed for a night or two at least. He will prefer to get along with three or four hours a day of sleep for a week if excellent nights should continue so long" (p. 201). Anyone endowed with this sort of spirit will derive valuable assistance from the book, whatever may be his instrumental equipment or line of research; and the information given is derived from the most competent sources, as the following list of subjects treated and authors will show:—

The Telescope ...	Sir H. Grubb.	Variable Stars......	J. E. Gore.
The Sun............	E. W. Maunder.	Star Colours.........	W. S. Franks.
The Moon	T. G. Elger.	Stellar Distribution	T. W. Backhouse.
The Planets	W. F. Denning.	The Zodiacal Light	T. W. Backhouse.
Comet-Seeking ...	W. F. Denning.	Meteors	W. F. Denning.
Double Stars......	S. W. Burnham.	The Aurora.........	J. R. Capron.

But besides a zealous spirit, some preliminary knowledge is presupposed in the amateur; and we are inclined to think that the editor has not been quite wise in the exercise of his discretion on this point. There are so many things which we should have liked to see included in the book, if only for the sake of completeness. The various articles are excellent in themselves, but were doubtless written independently; and the editor should have been on the alert to supply information which each author has left to another to communicate. As it is, the amateur, as somewhat curiously defined on page 1, is left uninstructed on such matters as the setting up of his equatoreal—the necessity of time—the working of his observatory; he is supposed to know a good deal about micrometers, charts to use in meteor observing, not to speak of such matters as refraction and position-angles. It is better to err on the side of telling too much or too often than on the other; and we should be inclined to think that it would be better to run the risk of being tiresome by saying a few words on such terms as Right Ascension and Declination rather than that of being so concise as to be unintelligible. If anyone thinks this expression too strong, let him try to make out the meaning of the method sketched on pp. 43–44. A few words of explanation of the notation would have made all clear; but they are wanting, and the result is a puzzle.

Such faults are the editor's. He has thought to make a book by stringing together excellent but incongruous chapters: his own preliminary chapter is not half big enough or should have been only one of several, though it is to be hoped that if he had accorded himself this licence he would not have multiplied such premature and somewhat misleading statements as those about the Lick telescope. Has he ever considered what the size of the emergent pencil would be with a power of 3500? (see p. 10). The choice of an eyepiece is another point on which the amateur is always asking for definite information and so seldom gets it.

He is recommended to use low powers; but why is it not pointed out that the emergent pencil should not be less than about 0·02 in. (to prevent the interference of diffraction) or greater than 0·20 in. (the size of the pupil of the eye), which immediately gives the two limits for a given aperture? There should be more notes like that on p. 34, which is excellent, as also Mr. Franks' chapter on Star Colours. We should have liked to see less of controversial matter in a book intended for instruction. Mr. Burnham is the chief offender on this head; but it is only fair to add that his paper was not originally intended for this work.

CATALOGUE OF NEBULÆ.—Dr. Dreyer has recently completed a most important work, viz. the collection into one general catalogue of all nebulæ of which observations have been published. This catalogue, containing particulars of 7840 objects, forms Part I. of Vol. xlix. of the Mem. R.A.S., and in view of its importance to observers we believe that a certain number of additional copies have been printed by the Society, and may be purchased at 15*s*. each. The work seems to have been done with a thoroughness worthy of Dr. Dreyer's reputation. Besides the mere collation of catalogues, the well-known inaccuracy of Herschel's General Catalogue, due to the rapidity with which many of the observations were made, has necessitated a careful revision of many places; and the dozen closely printed pages of notes which are given at the end of the catalogue show that this labour has been somewhat severe.

The least satisfactory part of the catalogue must needs be the estimations of brightness, which can only be taken as approximations. The observer's eye, the weather, and the instrument used, are all factors in these results. It is to be hoped that ere long some photometer may be adapted to the more accurate determination of the relative brightness of nebulæ.

WE have received the 'Annuaire de l'Observatoire Royale de Bruxelles' for 1888, the 55th year of publication. The general arrangement is the same as last year. Two original papers are contributed by members of the Observatory staff—one by M. Niesten on the results obtained for parallax of γ Draconis with the reflex zenith-tube at Greenwich, and the other by M. Fievez on the flame of a candle and its many interesting features. In the former M. Niesten refers to the fact that both Main's and Downing's reductions of the observations at different times give a negative parallax and exceptional values for aberration and nutation; and shows that the periodic source of error affecting the observations may possibly be identified with M. Folie's "diurnal nutation." He only takes, however, those 48 of the 266 equations used by Downing which relate to isolated observations; and we cannot attach much weight to this preliminary reduction, which is in no way comparable with those of Main and Downing.

The Asteroids.—In an excellent little pamphlet [*] dated November 1887, Prof. Kirkwood sums up what is known of the asteroids to date in a thoroughly readable form. In the few paragraphs he devotes to the question of their origin, and indeed throughout the book generally, he has been careful to avoid too much speculation, and almost rebukes himself for having been betrayed into a genial reference to the incautious person on Menippe, who takes a leap and finds that he has started on a journey of several hundred feet, and will be lucky to get back within the hour. The appended tables are very complete at present, but unfortunately cannot long remain so.

NOTES.

Solar Activity.—The following are Prof. Tacchini's published results for the year 1887 :—

1887.	No. of Days of obs.	Comparative frequence of spots.	Comparative frequence of days without spots.	Comparative area. Spots.	Comparative area. Faculæ.	No. of Groups of Spots per day.
Jan.	23	2·87	0·26	9·35	11·52	1·17
Feb.	23	3·35	0·26	7·83	10·09	1·32
Mar.	26	1·00	0·62	3·35	16·00	0·42
Apr.	25	1·12	0·48	7·76	6·80	0·68
May	28	4·18	0·25	22·04	9·29	1·11
June......	27	4·15	0·04	29·74	20·37	1·37
July	28	5·07	0·18	25·25	14·11	1·68
Aug.	28	4·60	0·36	23·53	14·29	1·32
Sept. ...	26	2·47	0·50	15·75	9·23	0·56
Oct.	19	1·27	0·47	20·21	10·53	0·70
Nov.	17	1·70	0·47	6·41	17·30	0·71
Dec.	19	6·68	0·16	40·10	16·84	1·21

Prominences.

1887.	No. of days of obs.	Mean number.	Mean height.	Mean extent.
			″	°
Jan.	18	6·4	44·2	1·6
Feb.	19	7·1	45·0	1·4
Mar.	22	6·3	45·2	1·6
Apr.	17	7·5	45·4	1·4
May	16	7·6	45·7	1·6
June	17	9·4	44·6	1·3
July	27	9·8	49·4	1·9
Aug.	24	9·4	45·9	2·0
Sept.	17	9·5	44·2	2·0
Oct.	11	6·3	39·0	2·1
Nov.	13	11·0	44·0	1·6
Dec.	13	8·3	44·2	1·6

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[*] 'The Asteroids, or Minor Planets between Mars and Jupiter,' by David Kirkwood, LL.D. Philadelphia: J. B. Lippincott & Company. 1888.

General Perrier, the eminent French geodesist, died on Monday, at the age of fifty-five. His work on the trigonometric junction of the English and French coasts is well known; and one of his last projects was to secure the cooperation of the Astronomer Royal and Admiral Mouchez in a careful redetermination of the difference of longitude between Greenwich and Paris, which it is hoped to carry out during the summer of the present year.

We hear that the energetic assessors of rates and taxes in a village-on-a-hill not far from London have claimed large sums from a distinguished amateur astronomer on the score of his telescope, which they regard as a "fixed plant." The suggestion has been made that if he unclamps it next time the despot comes round, and shows that it is movable, he may be able to soften his heart.

Mr. Frank Muller, of the Leander McCormick Observatory, requests that unpublished observations of Comet Barnard (1887, IV.) may be sent to him.

We are glad to see that Mr. Knobel has written to the 'Sidereal Messenger' with reference to the abusive and incorrect statements made in that Journal by Mr. Proctor with regard to the election of Prof. Holden as an associate of the Royal Astronomical Society; and that the 'Sidereal Messenger' has at once expressed its regret for the publication of a letter which they received in all good faith from a man of some eminence. We trust that Mr. Proctor may be induced to offer something in the way of explanation and apology, though it is difficult to understand how anything less than an unreserved withdrawal of his statements will meet the case.

The Council of the Royal Meteorological Society have arranged to hold at 25 Great George Street, Westminster (by permission of the Council of the Institution of Civil Engineers), on March 20th to 23rd next, an Exhibition of *Apparatus connected with Atmospheric Electricity*, including Lightning-conductors, Photographs of Lightning, and damaged objects.

We regret to announce the death of Dr. J. J. Jedrzejewicz, the distinguished amateur astronomer, who devoted the leisure moments of a busy doctor's life to observations of comets and double stars. He died at Plonsk on 1887 December 31, at the age of 52.

A Comet was discovered by Sawerthal on February 18; was observed at Capetown February $18^d\ 14^h\ 32^m.5$. Position:—

R.A.	$19^h\ 11^m\ 32^s.5$	Daily motion ..	$+7^m$
N.P.D.	$146°\ 4'\ 44''$	" ..	$-1°\ 15'$

The physical appearance of the comet is as follows:—7th magnitude, nucleus well defined; tail $= 1°$. It is visible to the naked eye.

Minor Planet No. 271 has been named Penthesilea.

Minor Planet No. 272, 13th magnitude, was discovered by M. Charlois at Nice on Feb. 4.

ERRATUM.

the 'Observatory,' No. 133, line 3, *for* solar parallaxes *read* stellar

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 135. APRIL. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, March 9, 1888.

W. H. M. Christie, M.A., F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel and Col. Tupman.

The Minutes of the previous Meeting were read and confirmed.

Col. Tupman announced that 101 presents had been received since the last meeting. Amongst those specially worthy of mention were the first instalment of the Paris Catalogue, which contained the reobservation of about half of Lalande's stars in first six hours of right ascension. The number of stars in this volume is 7245. Accompanying it was a volume containing the individual observations. From Harvard there has been received Vol. xiii. part 2, of the Annals, containing zone observations of 5500 small stars, made with the Transit Wedge-photometer by Prof. Pickering. The second volume of the Annals of the Observatory of Nice had also been received. From Dunsink a volume of observations of 1012 Southern stars, by Mr. Rambaut. From Bonn the last instalment of 12 star-charts of the Southern Durchumsterung. The Cordoba Observatory (Argentine Republic) sends us its Vol. ix., containing the observations of 1876; and Mr. R. Gautier, of Geneva, an investigation of the orbit of Tempel's first Comet. Mr. Ranyard had presented six volumes of 'Knowledge,' which made the Society's set complete. Mr. Common had presented the magnificent enlargement of his Orion Nebula photograph, recently exhibited at Manchester.

The President. I would ask you to return your thanks to the donors of these very acceptable presents.

Mr. Knobel. We have a large number of papers on the occultations and observations of the total Lunar Eclipse of January 28. Dr. Dreyer's paper contains observations made at the Armagh Observatory. He records seven disappearances and two reappearances, and he also calls attention to the fact that about the middle of the eclipse the moon appeared to the naked eye as a fairly bright disk, with a dark isosceles triangular shadow described on it, having the base to the north. That triangular shadow on the Moon's disc seems to have been noticed by others. Mr. Backhouse sends us a paper with a diagram showing the triangular form of the shadow about the centre of the disk, with a long description of the physical appearances which he noticed. Mr. Rambaut, of the Dunsink Observatory, sends us a very carefully made series of observations. He succeeded in observing 17 disappearances and 18 reappearances, and the observations appear mostly to be very good; they are made with very great care, and doubtless they will be of considerable value. Mr. Denning sends us a paper upon the total lunar eclipse, which is confined to a discussion of the cause of the red colour visible upon the disk, and in which he is inclined to think that the saturated air-strata absorbed the violet rings and extracted the red, and hence the vivid colour observed during a total lunar eclipse. This may be an explanation of the phenomenon. Father Perry sends us a paper which I will read, as perhaps being a typical one of the observations which were made. Father Perry observed eight disappearances and nine reappearances. (Mr. Knobel proceeded to read the paper.)

Capt. Noble. I should like to say, Sir, that I was particularly struck with the fact that, as viewed from my own station, the body of the Moon generally was of an almost chocolate-red colour. The limb at no period of the eclipse was at all red; that was a kind of silvery green. I do not see the least bit the theory of this. The red light was the most apparent. There was a deep chocolate-red in the middle of the Moon. The shadow which has been described here as, I think, a triangle seemed to me to be a kind of amorphous mass, as though the place had been smoked. I refer, of course, to that portion of the actual shadow of the Earth into which the rays of the Sun did not penetrate. It looked to me, as I have said, as though it had been smoked. I was particularly struck with the silvery green of the actual limb of the Moon at the time of totality, when the Moon's disk generally was at its reddest, and then there was the curious appearance of which I have spoken, as of a smoky patch in the middle of the lunar disk. While I am on my legs I may add, as an illustration of the exceedingly partial and local character of English weather, that while up to the time of totality I saw the whole of the phenomena most beautifully, the sky being brilliant, Mr. Prince, who is a Fellow of this Society, whose Observatory is situated exactly four miles and two-tenths as the crow flies from mine, and is 500 ft. higher (here comes the curious thing) above the sea-level than mine, had his sky covered with a mass of impenetrable cloud. Now if my sky had been covered with cloud, and he, 500 ft. higher, had seen the eclipse, it would of admitted of a very simple explanation; but that I, 500 ft. lower and only some four miles or so from him, should see the whole eclipse up to the time of totality brilliantly, and he not at all, does afford, it seems to me, striking illustration of the extraordinarily local character of English weather.

Mr. Turner. I think the absence of the C line in the spectrum was confirmed by observations made by Professor Piazzi Smyth, or under his superintendence, and also by Dr. Copeland. The green colour that Capt. Noble mentions in the limb, I think, was noticed in several other places. Several amateur observers have commented upon the difference between the colour of the limb and the centre, and they uniformly note the colour of the limb as greenish.

Mr. Chambers. I can confirm Capt. Noble's observation as to the patch in the centre of the Moon's eclipsed disk, and I think his comparison of it with the patchy appearance often presented by the surface of a piece of smoked glass is a very good one. Nothing that I saw was at all capable of being twisted, by any reasonable expansion of words, into the "triangle" spoken of by two or three previous speakers. Captain Noble described the colour as of chocolate. A deep red coppery chocolate no doubt would be a description which would accord with many previous records; but on this particular occasion it struck me as very remarkable that the colour was by no means of the customary deep tinge—so much so, that I should very much prefer to describe it as pink or pinkish.

Mr. Ranyard. I think there can be very little doubt, Mr. President, that the light upon the Moon's surface was not uniform; whether there was a triangular patch or not I do not know. There was a very irregular patch, and during other total lunar eclipses the irregularity of the light on the eclipsed Moon has been frequently observed. That, I think, is sufficient proof that the light is not coronal light. The bright part of the corona not being very large, compared with the Sun's disk, would not be visible all round the Earth's disk, which, as seen from the Moon, would appear about a couple of degrees in diameter; if the illumination was derived from the corona you would expect the light to be much more uniform within the eclipsed area. There is another physical reason which, I think, also makes this theory of coronal illumination quite out of the question. The light of the corona is certainly very faint indeed compared with the light of the terrestrial atmosphere, which hides the corona when the Sun is uneclipsed; there is evidence that the corona gives less than the two-thousandth part of the light from the atmosphere. And, of course, the light from our atmosphere close to the Sun's limb is very small indeed, compared with the direct light of the Sun. Now the brightness of the eclipsed Moon is not indefinitely smaller; it does not correspond to thousandths of parts to the light of the uneclipsed Moon in the penumbra just outside the total shadow. There is no doubt a sharp transition, but there is not a difference of thousandths of times. It is, I think, also evident that in the region of total shadow the light of the Sun must be seen through the Earth's atmosphere. We know this from the amount of the refraction at the Earth's limb. As far as I remember, the total shadow of the Moon is about $\frac{19}{20}$ths of what it ought to be if a geometrical shadow of the Earth were thrown and there were no refraction at all. That would give a refraction of, I think, a minute and a half on either side. There is the double refraction in going through the Earth's atmosphere at each limb, and there is the encroachment on either side we have to take into account. Let us take the total diameter of the total shadow as 2 degrees, or something less than that; of course it differs from eclipse to eclipse. The twentieth part of that would be six minutes in all, or three minutes on either side; and that would give the refraction in entering and leaving the Earth's atmosphere as 1½ minutes, which would correspond to the refraction at a very great height in the Earth's atmosphere; the grazing refraction would decrease the area of total shadow by over 2 degrees, for the refraction at the horizon would be doubled for light going through the Earth's atmosphere and grazing by the limb of the Earth. We therefore know that the light which illumines the dark Moon just inside the total shadow must have passed through our atmosphere at a great height, and there is no part of the lunar shadow which is a whole degree away from the edge of the circle of total shadow; light grazing the Earth's limb would be refracted more than a degree into the total shadow. Therefore an observer on any part of the Moon's surface, within the total shadow, would see the Sun through the Earth's atmosphere; he would see a rim of light round the Earth no doubt very red, for we see the Sun's light become red when it comes into the Earth's atmosphere so as to graze the horizon, and going out again it would become still redder. If that light is the light which lights up the total shadow on the Moon, we can conceive that it might be very irregularly dispersed; and such a triangular marking would probably mean that there was some irregularity of refraction at different parts of the Earth's limb, or an irregularity of absorption, which would give the same effect. If at one part of the Earth's limb there was greater absorption due to dust floating in the higher atmosphere, or to some cause varying the absorption or refraction of the higher parts of our atmosphere above the region of cloud, then we should have patches of brighter and darker light within what we ordinarily call total shadow of the Earth.

Mr. Knobel. I do not find in any of the papers we have received any remark about the limb being of a greenish colour. The observers generally seem to give the tint of the limb as yellow, straw-colour, red, and coppery; but in the papers that we have received I do not see any reference to the greenish tint described by Capt. Noble.

Dr. Spitta. I noticed the pink colour spoken of by Mr. Chambers, but the smoky patch appeared to me to be pear-shaped, not triangular at all. The pink was very vivid, both with the reflector and with the refractor.

The President. Perhaps I may be allowed to suggest a possible explanation of this green light round the Moon which occurs to me. It is well known that the edge of the Moon appears brighter than any other portion by virtue of the light being scattered from a rough surface. Anyone who looks at one of these photographs will see that the Moon is marked with a bright ring round the limb. The edge is bright, and there are also variations of brightness of the surface, which must largely affect the appearances during the eclipse of the Moon. A thing which struck me during this eclipse particularly, and also during previous ones, was that the first symptom of the umbra did not occur at the limb, but there was a sort of diffused cloud within the limb, and the limb was quite distinctly bright with a bright ring round, and that ring was visible for some time. Now, if the obscured part is of a coppery or pinkish colour, according to circumstances or to the eye of the observer, it seems natural that the brighter light round the edge will be tinged with the complementary colour green. If no other Fellow has any remarks to make, I will ask you to return your thanks to the Rev. S. J. Perry for this interesting communication. The other papers on the eclipse deal with occultations of stars, a matter of great interest from another point of view.

Mr. Knobel. Mr. Hartnup sends us a list of the occultations observed at the Liverpool Observatory, seven disappearances and four reappearances. The Glasgow Observatory succeeded in observing six disappearances and seven reappearances. The Oxford University Observatory seems to have been very unfortunate, the weather having been most unpropitious and fitful; they only succeeded in observing eight disappearances and five reappearances, and do not attach very much weight to them on account of the unpropitious weather. The Radcliffe Observatory, Oxford, succeeded in observing sixteen disappearances and four reappearances, but they were made under rather unfavourable atmospheric conditions, the sky becoming hazy and cloudy.

Col. Tupman read a paper on "Observations during the recent Eclipse." He stated that every precaution was taken to ensure success. The unexpected brightness of the Moon rendered the smaller stars quite invisible. He examined the computations sent him by Prof. Döllen, in which all the disappearances and reappearances, to the number of 74, were computed for his observatory, and were all quite accurate.

Referring to a statement made by General Tennant, that the Moon was of a very bright white colour, Col. Tupman said he could not agree with General Tennant that the Moon was white; it seemed to him to be very red, though less red when seen through a telescope than when viewed by the unaided eye.

The President. I may mention that at Greenwich preparations were made to observe the occultations of stars during the eclipse. Mr. Turner took a great deal of interest in the matter, and made various arrangements intended to secure the observation of the greatest number of stars possible in the shortest space of time; but unfortunately these arrangements were to a great extent frustrated by very cloudy weather, so that only a few occultations were observed during gaps in the clouds. Perhaps, as Mr. Turner is present, he might give the Meeting an account of the different arrangements which were adopted, as some arrangements of that kind, I think, would be useful in similar conditions in future.

Mr. Turner. The first step that was taken was to check the times for occultation which had been sent by Professor Struve by means of his catalogue of places, and I must concur with Col. Tupman in saying that these places were found to be extremely accurate, and had obviously been computed with very great care. There was not a single mistake found in the whole list of 30 times that we checked. In the second place we gave particular attention to methods for securing the observations of these stars with rapidity in the dark. From previous experience during the eclipse in October 1884, it seemed advisable to prepare our instruments in such a way that we should not have to use a lamp at all, because the stars were so faint that it was necessary to keep the eye sensitive throughout the observations. One method used at Greenwich was to adapt a temporary position-circle of cardboard, which was graduated with a sufficient degree of accuracy for the time being, and to cut notches in it at the important points. I may explain that there were at Greenwich ten stars of which the disappearances and reappearances could be observed. That gives five important points on the right-hand limb, and five important points on the left-hand limb, which is not a very large number. At these points notches were cut in the cardboard position-circle, and a flexible pointer was fixed to the eyepiece which would fall into these notches, and which could be felt to be in position with the fingers. This was found to work very satisfactorily. The position-circle could be set and the eyepiece turned to the right position very quickly without using any light at all. Mr. Maunder used, perhaps, a more ingenious device still. He painted his position-circle with luminous paint, and was very well satisfied with the result of that. Another device which was used by Mr. Maunder, and which we found could only be used on two of the equatoreals, was to fix the telescope on the centre of the Moon and trust to the driving-clock; and then to make the eyepiece travel round the limb, not mounted centrically as usual, but mounted eccentrically, so that by turning it on the pivot it would travel all round the limb of the Moon. That was also found to work very successfully, this arrangement being easily secured by means of a wooden block mounted temporarily at the eye end. I believe Mr. Stothert, of Bath, used the same device. It was very unfortunate we had such cloudy weather. The result of experiments showed that stars down to the 11th magnitude are certainly observable with a considerable degree of accuracy in these occultations.

Mr. Crossley. I am sorry to say our experience at Halifax was similar to what you had at Greenwich. Professor Struve calculated the places of the stars for Halifax. We have made observations of the same kind on the occasion of a previous eclipse. I would simply say that although the sky was very clear indeed up to between 7 and 8 o'clock in the evening, it afterwards clouded over with considerable density and entirely prevented the observations.

Col. Tupman. Mr. Turner has mentioned a variety of devices for securing as many of the occultations as possible; they come on so fast that unless you have some special device you lose half of them. I should like to mention the method General Tennant adopted. We used a low power, the field of which was a little more than the diameter of the Moon, and across the diaphragm I placed five equidistant wires, dividing the Moon's diameter into four equal parts in declination. The ordinary angles of emersion and immersion were converted into differences of declination, measured from the Moon's north limb, and expressed in units of the distance of the wires. The number being called by an assistant, the observer was able to direct his attention very nearly to the correct point of the limb. When the stars were very near the north or south limb, the angles were retained in preference to this declination ordinate.

That was very successful as far as it went, but the work was interfered with by the clouds.

Mr. Turner. I suppose you used an extremely low power with large field?

Col. Tupman. Yes, that is the only way.

Capt. Noble. But, then, had you calculated the reappearances?

Col. Tupman. Oh, yes! every star.

Capt. Noble. You knew roughly where to look for it.

Col. Tupman. The declination ordinate enabled me to look almost exactly in the right spot.

The President. I may say with regard to the accuracy of the computations made at Pulkowa, that Mr. Rambaut sends the results of his observed times compared with computed times, and he says:—"It will be observed that only in ten cases out of thirty-five is the difference greater than two-tenths of a minute, which is about the limit that Struve expected." The greatest difference that he noted was in one star, where the observed time differed by one minute from the computed time.

Mr. Dunkin. I was exceedingly interested in Mr. Struve's suggestion for the observations of occultations of small stars during the late total eclipses of the Moon, particularly that of 1884, and if the sky had been clear at the last eclipse I think we should have had some very interesting observations. But what I wish more particularly to say now is not altogether relating to these two eclipses, because I am not in a position to know much about them, except as an outsider. In old times, of course, it has been my duty to observe many lunar eclipses, though in a different style to what we are now accustomed, as we never thought of utilizing a lunar eclipse to determine the diameter of the Moon, which we see can now be practically done by the observation of a sufficient number of occultations of stars at the eclipsed limbs. In former years it was thought sufficient to observe the mean solar or sidereal times of the beginning and ending of a lunar eclipse, and perhaps also the times when the penumbra came in contact with the principal spots, and at the Royal Observatory it was considered that the observers were well employed in observing the times when the shadow approached these separate spots. Compared with the Pulkowa plan these seem to be very crude observations now, but I remember very distinctly that we used to think it important to obtain them. In those early days of mine these observations were thought to be the best that could be obtained from a lunar eclipse. But with regard to the late eclipse, I think that it is a very great loss to astronomy and to science generally that the sky was so clouded at Greenwich. I knew that the careful preparations, made according to the Pulkowa plan, would be carried out so well at the Royal Observatory, that I was looking forward with the greatest confidence to the prospect of obtaining a series of observations there by the Astronomer Royal, which would be satisfactory, especially to M. Struve, who has taken so much interest in the matter. When the eclipse was progressing, though interested in much that I saw, the principal reflection that I could gather scientifically from the phenomenon was one of intense disappointment; for though I was located at my own residence, my heart was in the Observatory all the time, and I could not help, as the minutes passed by, wondering and hoping that my late colleagues would be able to secure some important observations during the short intervals of clear sky. But unfortunately very little of value appears to have been obtained; but that little, I hope, when added to the numerous observations of other observers—as I see that a great many have been comparatively successful—will enable M. Struve or M. Döllen to glean some valuable information concerning the true dimensions of the Moon, and thus obtain some data which will prove to be beneficial to astronomy.

Mr. Knobel. There are some photographs of the total eclipse by Mr. Peck handed round the room. Mr. Peck submits therewith a paper in which he refers to measurements he has made, which lead him to think that while the middle of the eclipse is as determined by the 'Nautical Almanac,' the beginning of it is earlier, and the ending later than the times stated. I do not know whether that will be supported by other evidence—that there is error in the time of the eclipse. Mr. Peck inclines to think that the photographs are capable of measurement, so as to determine the correct time of the commencement and termination of an eclipse.

Mr. Bryant. Does Mr. Peck give the errors?

Mr. Knobel. Yes, he gives the times from the 'Nautical Almanac.' The photographs show that the commencement was

$2^{m}\ 10^{s}$ earlier than $10^{h}\ 31^{m}$, that the commencement of totality was three minutes earlier, and the termination of totality given at $12^{h}\ 9^{m}$ was $3^{m}\ 2^{s}$ later; and the end of the eclipse given at $13^{h}\ 9^{m}$ was $2^{m}\ 4^{s}$ later.

Mr. Bryant. As you remark, I think the determination of the time by photography would be difficult with such an ill-defined object as the Moon's shadow. But possibly some part of this error, if error really exists, may be due to the erroneous value of the allowance for refraction in the passage of the Sun's light through the Earth's atmosphere to the Moon, and possibly partly to the spheroidal form of the Earth. I believe that a mean latitude of 45 degrees is adopted in the 'Nautical Almanac' office, at any rate extreme accuracy is not required.

Mr. Knobel. I think Mr. Peck seems to make very little allowance for the very ill-defined boundary between the penumbra and the umbra. In practical observation it is an exceedingly difficult thing to tell within a minute or two when the actual shadow as distinguished from the penumbra gets upon the Moon. It is almost impossible.

The President. There is just one point I may mention. There must necessarily be a great difference in the perception of the commencement of totality or anything of that kind between a photographic plate and the human eye. The photographic plate being insensitive to the red rays which pass through to the Moon's surface in an eclipse, naturally there would be what might be called a personality in the photographic plate which would make computations derived from measurements of photographs hardly comparable with the result of observations by the eye. I merely throw out that suggestion as possibly explaining the discrepancy.

Mr. Common. I should like to remark with regard to the determination of phases by means of photography, that some years ago when working with an 18-inch reflector during a total eclipse of the Moon, I took a series of photographs and found this very remarkable thing occurred, that as the shadow was progressing over the Moon, if I took a plate and gave it a short exposure I got different results from a plate that was taken later with longer exposure, so I could alter the phase by prolonging the exposure of the photograph. It struck me that photographs of an eclipse of the Moon were utterly valueless to determine the phase unless they had all been taken on some rigid plan, and were compared with the eye-observations, and then they would be more useful to determine the difference between the apparent colour and the actual photographic result. I tried some experiments afterwards by means of a shade over the negative where I was printing, and I got exactly the same results to all intents and purposes by an artificial eclipse of the negative when printing.

The President. I would ask you to return thanks to the authors of these papers for their communications. I need hardly remark on the importance of this method of determining the two dimensions of the Moon as unaffected by irradiation or any disturbing causes. It is a matter of extreme importance; and it is only to be regretted that cloudy weather has interfered to a certain extent with the accuracy which might otherwise have been attained.

Mr. Knobel. We have an interesting communication from Dr. Doberck on the Telegraphic Determination of the Longitude of Haiphong.

The President. I will ask you to return your thanks to the author of this paper. The determination of telegraphic longitudes in the east is a matter of very great importance in the interests both of astronomy and navigation.

Mr. Dunkin said he had received a paper from Professor Glasenapp, of St. Petersburg, on the Orbit of λ Ophiuchi, with a request that he would communicate it to the Royal Astronomical Society. Though the paper was a short one, he thought it was very important; because anything which tended to collect the errata of the original standard observations of double stars, or of any other class of astronomical observation, was of great importance. Prof. Glasenapp's paper gives a new explanation of the evidently erroneous angles of position recorded in Sir William Herschel's observations of this star (Phil. Trans. 1804), which have puzzled several of our principal double-star observers, by whom various attempts at interpretation have been made. Assuming that the error originated in a mistake in writing, Prof. Glasenapp forms a series of 16 combinations of possible readings, among which the true angles must be found; of these only one gives a satisfactory explanation. Taking these adopted angles, he has computed a new set of elements of the orbit of this binary, which satisfies the observations made at different epochs, all the residuals being very small.

Mr. Sadler said Sir William Herschel never measured anything so small as half a second. The smallest distance he ever measured was $2\frac{6}{10}$ secs.

The President. I would ask you to return your thanks to Professor Glasenapp.

Mr. Crossley said that last year he had the pleasure of describing a small centering-tube that he constructed for facilitating the centering of the mirrors of the large telescope. The centering-tube was 10 in. long and $2\frac{5}{8}$ in. in diameter, with an eye-hole at one end, and at the other end two interchangeable diaphragms—one with an aperture just large enough to include the large mirror, the other just large enough to include the flat as reflected by the large mirror. There was this disadvantage about the tube, that whereas it could be very easily used in the daytime, it could not be used so well at night; and also, if the flat was not symmetrically mounted, the centre of the flat would not correspond with the centre of the mounting as reflected by the large mirror. He had since altered the tube so as to be available for use at any moment at night. The tube was of exactly the same length; but there was an open elbow, the full diameter of the tube, at the eye-end. In the tube opposite the elbow was placed a reflector at an angle of 45°, with an opening in the centre sufficiently large to enable the eye to see the whole of the diameter of the tube at the other end, where there was placed a disk 1 in. in diameter, with a ¼-inch hole in the centre. This disk was supported by four arms attached to a rim sliding into the tube.

The first operation was to centre the eye-tube upon the flat. That was done by seeing that the flat appeared symmetrical when the eye looked at it through the aperture. In the centre of the large mirror there was gummed a small disk of paper, say half the diameter of the centering-tube. This disk was seen in the centre of the ¼-inch hole. By day it appeared white, and at night a lamp with a bull's-eye was held to the elbow, and the light thrown down the tube, and an image of the whole aperture of the tube was reflected back by the surface of the large mirror surrounding the disk of paper, and thus giving the effect of an annular eclipse when the mirrors were properly adjusted, the paper disk appearing dark. After centering the eye-tube on the flat, the flat was adjusted so as to bring the disk on the large mirror central, and then the large mirror was adjusted so as to bring the reflected image of the illuminated eye-tube concentric, and then the mirrors were perfectly adjusted.

Both the telescope and the dome were in good working order. By means of two cords controlling a water-engine, the dome could be moved round either way in any position of the instrument from the eye-end of the telescope. This was found a very great convenience; the dome could be moved either quickly or slowly, as required. With regard to the handling of the large mirror for silvering, he had made a great many experiments. He had placed the mirror vertically in a trough, and found that did not answer on account of the silver precipitate descending rapidly and interfering with the deposit. He also tried mechanical means for holding the mirror; but he had now gone back to Mr. Common's original plan of the vacuum box. He found, by experiment, that about 2 in. of mercurial vacuum would just hold the mirror; on doubling it to 4 in., he found that the mirror suffered no injury in form from that pressure. Mr. Crossley also spoke of the manner in which the flats were tested, and expressed his intention of giving the Society a more complete account of the dome for publication.

Mr. Common said, with regard to the question of silvering, of course he was pleased to hear that after all Mr. Crossley had come back to the old plan which he himself had used, and which he had found very successful. With regard to the adjustment of the telescope, there might be differences of opinion as to what was and what was not important. Many people seemed to think that the tube was something more than a mere accident, and that in some way it was part of the optical arrangement. From the fact that most telescope-makers made the tube of the reflecting-telescope very nearly the size of the cylindrical pencil of light falling on the mirror from the star, the position of the tube had to be taken into account; and as the support of the eye-end it was useful, but it ought to be left out of account otherwise; in considering the optical adjustments there were other matters, such as the flat being at exactly 45° with the tube, that were equally unimportant though usually insisted upon. Mr. Common showed on the blackboard the necessary points in the adjustments of a Newtonian telescope.

The following papers were announced:—

W. Doberck. "Telegraphic Determination of the Longitude of Haiphong."

J. E. Gore. "On the Orbit of 70 (p) Ophiuchi."

J. E. Gore. "Observations on the Variable Star S (10) Sagittæ."

Prof. S. Glasenapp. "Orbit of the Double Star λ Ophiuchi."

Rev. S. J. Johnson. "Southern Double Stars."

A. Marth. "Ephemeris for Physical Observations of the Moon for the nine lunations from April 12 to the end of December 1888."

A. A. Rambaut. "The Total Eclipse of the Moon, 1888, Jan. 28, observed at Dunsink Observatory."

J. Hartnup. "The Total Eclipse of the Moon, 1888, Jan. 28, observed at Liverpool Observatory, Bidston, Birkenhead."

J. L. E. Dreyer. "The Total Eclipse of the Moon, 1888, Jan. 28, observed at Armagh Observatory."

Rev. S. J. Perry. "The Total Eclipse of the Moon, 1888, Jan. 28."

W. F. Denning. "The Total Eclipse of the Moon, 1888, Jan. 28."

Admiral Sir E. Ommaney. "The Total Eclipse of the Moon, 1888, Jan. 28."

Lieut.-Col. G. L. Tupman. "The Total Eclipse of the Moon, 1888, Jan. 28, observed at Harrow."

W. Peck. "Photographs of the Total Lunar Eclipse of 1888, Jan. 28."

T. W. Backhouse. "Total Eclipse of the Moon, 1888, Jan. 28."

Glasgow Observatory. "Total Eclipse of the Moon, 1888, Jan. 28." (Communicated by Prof. R. Grant.)

Oxford University Observatory. "Total Eclipse of the Moon, 1888, Jan. 28." (Communicated by Prof. C. Pritchard.)

Radcliffe Observatory, Oxford. "Total Eclipse of the Moon, 1888, Jan. 28." (Communicated by E. J. Stone.)

Arthur Berry, B.A., King's College, Cambridge, and *The Rev. Edward Stewart, M.A.*, 1 Bank's Terrace, Croft-on-Tees, Darlington, were duly elected Fellows of the Society.

The following gentlemen were proposed for election as Fellows of the Society:—

Thomas Dallmeyer, Bloomsbury Street. (Proposed by Capt. Abney.)

Geo. Wilson Heath, F.R.G.S., Bexley, Kent. (Proposed by J. R. Jones and O. T. Olsen.)

Alfred Robert Johnson, M.A., St. John's College, Cambridge. (Proposed by H. H. Turner.)

Reginald Carter Nelson, 19 Roker Terrace, Sunderland. (Proposed by C. Hutchinson.)

Sir Henry Thompson, 35 Wimpole Street, W. (Proposed by A. A. Common.)

John Walther, M.D., C.M., West House, Eastbourne. (Proposed by J. Browning.)

Meeting of the Liverpool Astronomical Society.

March 12, 1888.

J. Gill, F.R.A.S., *President*, in the Chair.

The sixth meeting of the present session was held at the Royal Institution. Fifty-three candidates were elected and proposed, and Professors C. A. Young and E. S. Holden were elected Associates of the Society.

The Secretary announced that he had received letters from the members of the Council sympathizing with Capt. W. Noble, whose residence had been entirely destroyed by fire. The chairman remarked that few persons had done so much to popularize astronomy as Capt. Noble, and he was sure that the intelligence of his loss will be heard with regret by all amateurs.

Mr. F. Woulfe Brenan proposed that the society should institute periodical examinations in astronomy, and that the Council should be empowered to grant certificates of competency. The possibility of gaining such a certificate would stimulate amateurs to follow up some systematic study of the science, and prevent them from lapsing into mere stargazing. Mr. W. H. Davies seconded the proposal, but suggested that the certificate should not be granted for mere textbook knowledge so much as for practical experience. The Directors of Sections were likely to know who had done the most useful work, and they might include, in their annual reports, the names of those whom they thought deserving of certificates. Mr. W. F. Denning thought the idea was a good one. Many persons felt a shade of contempt for the assumed scientific dignities of men who strung a lot of letters after their names, but who had to pass no examination whatever for proficiency in the science they professed. If the Liverpool Astronomical Society would take the initiative in this matter, it would be a good thing in his opinion. The resolution, according to custom, was left for decision until the next meeting, a hope being expressed that in the meantime members would communicate their opinions by letter.

In continuation of his article on Plato, Mr. Elger described the remarkable details which have from time to time been detected on the floor—viz., light spots of a doubtful nature, crater cones, and delicate markings. He pointed out the physical connection which probably exists between these objects and the changes in visibility which they undergo. The author also referred to the interesting region north of Plato, and to the details of the great fault which traverses the formation from N.W. to S.E.

In concluding his paper on Jupiter, in the series of "Illustrated Notes on the Planets," Mr. Denning observed that drawings of that planet obtained under the highest powers that can be used with advantage, and with a cautious regard to faithful delineation, would probably throw much light on the phenomena occurring in the Jovian atmosphere. It was most desirable to pursue the various markings year after year with unflagging perseverance, for it was only by such means that we could hope to solve the extraordinary problem which their behaviour offered for solution. Too much stress could not possibly be laid on the necessity of observers being as precise as possible in their records.

In a paper on the double star Σ 175, Mr. Gore said that the changes in position-angle and distance in the case of the components of this pair was due to uniform rectilinear motion. Assuming that the brighter star was the one in motion, the proper motion was 0″·1198 per annum in the direction of 196°·8.

Papers were read by Mr. W. H. S. Monck on "Eclipses of the Moon"; by Mr. Elger on "Saturn"; by Mr. Tarrant on "Micrometrical Measures of 200 Double Stars"; and by Mr. St. Quintin Gage on "Star Colours."

An Educational branch was formed at Douglas, Isle of Man, which already numbered 17 members, and promised a rapid extension through the island.

The meeting terminated with an address to the students on the positions of the planets by Mr. J. Gill.

Meeting of the Royal Meteorological Society.

Wednesday, March 31, 1888.

Dr. W. Marcet, F.R.S., *President*, in the Chair.

Dr. G. E. Scholefield and Col. W. S. Young were elected Fellows of the Society.

The President (Dr. Marcet) delivered an address on Atmospheric Electricity. He first alluded to Franklin's experiments in America in 1752, who succeeded in obtaining the electricity of a storm-cloud by conducting it along the string of a kite sent into the cloud. De Romas, in Europe, repeated the experiment, and having placed a wire within the twine his kite was attached to, obtained sparks of nine or ten feet in length. The characters of the two kinds of electricities were next described—the vitreous or positive, which was produced by rubbing glass, and the resinous or negative, obtained by rubbing sealing-wax or another resinous substance; and it was shown, by bringing suspended balls of pith within the influence of these electricities, that electricities of different kinds attract each other, and those of the same kind repel each other. De Saussure's and Volta's electroscopes were next described, pith balls being used in the former and blades of straw in the latter for testing the pressure of electricity. With the object of measuring the force of electricity, Sir W. Thomson's electrometer was mentioned, in which the electricity is collected from the air by means of an insulated cistern letting out water drop by drop, each drop becoming covered with electricity from the atmosphere, which runs into the cistern where it is stored up, and made to act upon that portion of the instrument which records its degree or amount. The atmosphere is always more or less electrical, or, in other words, possessed of electrical tension, and this is nearly always positive; while the Earth exhibits electrical characters of a negative kind. The effects of atmospheric electricity were classed by Dr. Marcet under three heads:—1. Lightning in thunder-storms; 2. The formation of hail; 3. The formation of the Aurora Borealis and Australis. He explained how clouds acquired their electrical activity by remarking that clouds forming in a blue sky, by a local condensation of moisture, became charged with positive electricity from the atmosphere, while heavy dark clouds, rising from below nearer to the earth, were filled with terrestrial negative electricity, and the two systems of clouds attracting each other would discharge their electricity, giving rise to flashes of lightning. In some cases a storm-cloud charged with positive electricity would approach the earth, attracting the terrestrial negative electricity, and when within a certain distance shoot out a lightning which would apparently strike the earth; but it would just as well have struck the cloud, only there was nothing in the cloud to sustain any damage, while on the earth there were many objects a lightning would destroy, to say nothing of its effects upon animal life. Thunder is the noise produced by the air rushing in to fill up the vacuum made by the heat of the lightning flash. There may be sheet-lightnings, zigzag, or forked lightnings, and globular lightnings. The latter are particularly interesting from their assuming a spherical form. Illustrations were given of objects struck by lightning, the most remarkable being, perhaps, the clothes of a working-man which were torn into shreds, while the man himself was not seriously injured.

Dr. Marcet next proceeded to show a flash of lightning, which he produced by throwing on a white screen the image of an electric spark 2 or 3 inches in length, enlarged by means of the lens of an optical lantern; forked lightning, 6 or 8 feet in length, with its irregular zigzag course, was most clearly demonstrated. After alluding to the protecting power of lightning-conductors and their construction, Dr. Marcet explained the formation of hail and of waterspouts, and exhibited an instrument by Prof. Colladon, of Geneva, for showing the formation of waterspouts. He concluded his address with a few remarks on the Aurora Borealis and Australis, the formation of which was illustrated by De la Rue's experiment, which consisted of successive discharges of electric sparks through a partial vacuum while under the influence of a powerful magnet; electric sheets of light were seen assuming the form of bands and possessed of a certain rotating motion.

Mr. G. J. Symons, F.R.S., read a short communication on "The Non-existence of Thunderbolts," and briefly described the history of several so-called thunderbolts, the specimens obtained being of an amusing character, thus clearly showing that they were of a terrestrial and not a celestial nature.

In connection with this Meeting a most interesting Exhibition of Instruments was arranged in the rooms of the Institution of Civil Engineers. The Exhibition was devoted chiefly to Instruments connected with atmospheric electricity. There were various forms of electrometers, including those formerly in use at the Greenwich and Kew Observatories. Numerous patterns of lightning-conductors were exhibited, together with models of churches, houses, chimney-shafts, and ships showing the various methods of protection. The Postal Department showed a number of lightning-protectors used for Telegraph purposes. Many objects damaged by lightning were exhibited, including lightning-conductors, telegraph apparatus, portions of rafters, trees, &c., also the clothes of a man torn off his body by lightning. An interesting collection of meteorites and some alleged thunderbolts were shown, the latter being of an amusing character. There were also several new meteorological instruments exhibited, which had been brought out during the past twelve months.

One of the special features of the Exhibition was a most valuable and interesting collection of over fifty photographs of lightning-flashes. Many of these were taken during the great thunder-storm which occurred in London on August 17th last year, while others were taken in various parts of the world.

The Exhibition also included a large number of photographs of damage by lightning, and photographs of clouds and meteorological instruments, as well as records of atmospheric electricity, &c.

Meteor Notes.

A CONTINUANCE of very unfavourable weather has seriously interfered with observation during the past few months. In March the sky was almost invariably overcast, and very few meteors could be seen. On the 4th, between 11^h and $13^h\ 15^m$, 9 were registered by Mr. D. Booth at Leeds, including a small fireball as bright as Jupiter. The time of its appearance was $13^h\ 11^m$, and its path was from $205° + 13°$ to $190° - 2\frac{1}{2}°$, which it traversed rather slowly, leaving a very thin streak. The light of the meteor attained two maxima during its flight. The radiant-point was probably in the northern region of Hercules or Cygnus, and the meteor may have belonged to a shower seen at Bristol on the morning of March 15, 1877, at $263° + 48°$.

On March 3, a few minutes after 9^h, a brilliant meteor was seen from many places in the north of England. Several descriptions of its appearance were published in the newspapers, but unfortunately, as is too often the case, the observers omitted to give definite positions. At Wolverhampton, however, the meteor appears to have been well recorded by Mr. Joseph Hough, who gives the time as $9^h\ 2^m$, and the path as in a line joining ζ Ursæ Majoris, θ Draconis, and on towards α Cygni, or approximately from Azimuth 45° E. of W., alt. 35°, to Azimuth 20° W. of N., alt. 10°. Although shining through clouds which partially enveloped the sky, the meteor was exceedingly bright. One writer says the meteor made a great hissing noise as it passed down the heavens, and frightened a poor tramp who happened to observe it. It is much to be hoped that some further particulars of a useful character will be obtained concerning this meteor. Its direction, as noted at Wolverhampton, shows that it probably emanated from Leo. "In March, fine meteors diverge from S. of β Leonis" ('Monthly Notices,' xliv. p. 300), and its appearance on the 3rd of that month confirms the statement that during the first four days of March fireballs are very numerous ('Observatory,' iii. p. 129).

Meteoric Stones and Shooting-stars.—'L'Astronomie' (March 1888) mentions the probable identity of origin of these phenomena, and says that on August 10, 1885, the date of *rencontre* of the Earth with the meteor-stream connected with Comet III. 1862, a fall of meteorites occurred in the commune of Grazac, Tarn (France). On November 27, 1885, the date of our planet's passage through the meteors of Biela's comet, a meteorite fell at Mazapil, in Mexico. These coincidences are interesting. Instances have not been wanting in former years as to the fall of meteoric fragments on nights signalized by the occurrence of cometary meteoric showers; but these contemporary events have been usually regarded as accidental, and by no means traceable to the same derivative sources. It is often very difficult to determine the paths of meteorites with sufficient exactness to say whether they can be associated with the cometary streams. In many cases the evidence is opposed to the assumption that the latter furnish the meteoric stones and irons that have been recorded on the same date. Of the immense numbers of shooting-stars and bolides observed, from the special displays of April, August, and November, not one has been seen to fall to the Earth, and to have afforded the materials for proving that meteorites are derived from the cometary systems. The two instances alluded to above may throw a new light on the subject, and warrant the assumption that aerolites and falling stars are very similar in their physical relations. W. F. Denning.

Selenographical Notes.

Schickard.—This magnificent circumvallation, almost rivalling in size some of the smaller Maria, were it not for its position, which causes its nearly circular outline to be foreshortened into a more or less elongated oval, would be one of the most interesting formations on the Moon's visible surface, including, as it does, such a multiplicity of objects of various kinds on its intra-mural area of 13,000 square miles and associated with its wide and intricate border. Under those conditions of libration most favourable for exhibiting the interior at its maximum breadth, a number of crateriform objects are noted on the eastern quarter of the floor at no great distance from the foot of the wall. Among these *c* and *d* in Neison's Map xvi. are the most conspicuous; but there are many others of considerable dimensions, which neither Mädler nor Neison show, that are only to be found in Schmidt's large chart. It is noteworthy in connection with these objects that nearly all of them are associated with ridges, crater-rows, and short rills. The best example of this association is furnished by the craters *c* and *a* (on the south side of the floor), which, as drawn in all the maps, are connected by a long winding ridge; but they do not show a rill, which undoubtedly exists, running at the foot of the ridge on its western side and following all its windings. It commences at the south wall of *c* as a coarse furrow and, soon becoming narrower, gradually diminishes in width till, in the neighbourhood of *a*, its true character is only manifested with high powers in steady air. The direction of this cleft is prolonged beyond *c* in the form of a ridge running in a northerly direction to a coarse crater-row not far from the north-east border, and, as I found at 9^h on 27th November 1887, ultimately by a wide valley intersecting the north-east wall. On this occasion libration was very unfavourable for observing objects on the eastern section of the floor; but one could not fail to be impressed with the idea that the combined rill and ridge between *a* and *c*, the ridge north of *c*, and the valley traversing the border (all represented by an unbroken dusky line) are really the outward manifestations of a common line of disturbance extending from the crater *a* on the south to the plain north-east of Schickard. East of the ridge and rill connecting *a* and *c* is a third large crater lying just under the south-east wall. It is omitted by Mädler and Neison, but shown prominently by Schmidt, who represents it as nearly equal in size to *a*. From a common origin on the southern rim of this object two rills branch out, the more westerly following a serpentine course to the south-western border, where it passes between the two large craters which occupy so large a space thereon. The other pursues a still more winding path and ultimately abuts on the wall at a point some distance to the east of the two large craters. Schmidt draws a complicated system of crater-rows nearly on the site of these features, but with a power of 350 on a 8½-in. Calver on 2nd September 1884, under the best conditions, they both appeared to me to have the clean-cut and sharply defined character of true clefts, except for a short distance in the vicinity of the crater where they originate—here they were much coarser and less uniform in width. Immediately east of *c* are two short and nearly parallel clefts in close contiguity. The more westerly commences at a large, ill-defined, dusky depression without a visible rim, and, running in a northerly direction for about thirty miles, comes to a sudden termination. The other originates on the south at a small crater north-east of the dusky depression and ultimately almost coalesces with the first at its northern end, it being possible only during spells of the best definition to observe the two clefts here as distinct objects.

A careful examination of the floor of Schickard, especially of the northern half, would, I believe, under favourable conditions, reveal the existence of many more minute craterlets than have hitherto been recorded. Though Schroeter (Sel. Top. Frag. t. lxx. fig. 43) sets down four, Mädler only one, and Neison three, Schmidt has drawn sixteen, and on various occasions I have noted as many as twenty-five north of an imaginary line crossing the interior through the large craters *b* and *c*, and have glimpsed several others. In fact this area seems to be pitted with craterlets, many of them occurring in pairs. There are also numerous small light spots, apparently of a similar type to those in Plato and Archimedes. Most of the craterlets are very difficult objects, but four or five, though generally exhibiting no difference as regards visibility from others in the neighbourhood, become very conspicuous at full moon—a time when the rest are no longer traceable. It is noteworthy that the three which are the brightest under these circumstances are situated on a narrow rift or light streak which traverses the northern dark area—well shown in Neison's Map xvi., though he omits the objects which figure upon it and gives only one crater-pit (*m*) on the entire area. It is curious that in neither of his two drawings of Schickard does Schroeter show the large craters *b* and *a* on the west and south sides of the floor respectively, though he draws the principal craters near the eastern border and the minute objects just mentioned, which were all overlooked by Mädler and Neison and only a few of them seen by Schmidt.

The large dark areas and small dusky patches in the interior are worthy of more attention than they have hitherto received. The most remarkable is that which overspreads nearly the whole of the northern portion of the floor from Lehmann southwards to an imaginary line crossing the formation due east from Drebbel. Its southern limits are marked by a very irregular line, and in one place, as has been already mentioned, the dark area is traversed by a light streak. On the south-western quarter of Schickard there is another well-marked dark area extending from the border to the west side of the crater *a*, where its two perfectly straight sides form a right angle. This feature is visible soon after sunrise, before the larger area on the north can be distinguished, and under a higher sun becomes considerably altered in shape and extent, apparently retreating towards the border. At 9^h on November 27, 1887, when the east wall of Schickard was on the morning terminator, there was no trace of either of these areas and scarcely any contrast of tone was perceptible on the floor except such as was manifestly due to the difference in the Sun's altitude on the two sides of the formation. Among the small dusky markings the most remarkable is that which runs in the form of a narrow streak from the north-east side of the crater *b* and joins the northern dark area. At one place it exhibits a blunt projection and it is worth noting that a small craterlet is included by the protuberance. It is impossible to say what amount of care was bestowed by Mädler and Schmidt in the delineation of the boundaries of these dark areas, or whether their representations of them are not to a certain extent conventional, but they now present a very different outline from that shown in their maps. It would be a promising and easy task for amateurs to make careful drawings of them, under various conditions of illumination and libration, with a view to provide subsequent observers with reliable data by which to test the progress of possible changes in the future. Some of the numerous craterlets, if carefully mapped, afford excellent points of reference.

Occupying a considerable space within the true border on the north side of Schickard and extending up to the west of Lehmann, are found what Neison terms "a number of rounded peaks on a level plateau,"—marked *i* in his map. Viewed when libration is favourable, these objects impress one with the idea that a vast landslip has taken place here. This is rendered more probable by the fact that the actual border, which runs outside the plateau, is here much narrower and less lofty than elsewhere, and the surface of the plateau appears, from the shadows cast by mounds upon it, to slope gradually to the floor, where its very irregular boundary, marked by a number of blunt projections, is easily distinguished.

Kempston, Beds, 19 March, 1888. THOMAS GWYN ELGER.

Variable Double Stars.

THE circumstance that variable double stars show, for the most part, a spectrum of the Sirian type is rendered more significant by the apparent inability of single stars of that description to fluctuate sensibly in brightness. Algol-variables, if the eclipse-theory of their changes be admitted, make no exception to this rule. They indeed, since most (perhaps all) of them emit light strongly modified only by hydrogen-absorption, add to the weight of hidden meaning it carries.

Apart from them, Gore's Catalogue includes three stars—Y Virginis, S (15) Monocerotis, and δ Orionis—belonging to the first spectral class *. All are compound objects. The physical nature of the last-named pair is indeed open to much doubt. A second and a seventh magnitude star 52″ apart cannot be presumed to be really connected; and no evidence bearing on the point is at hand beyond that of relative fixity during upwards of a century. Nor is there proper motion enough to render this significant. That of about 2″ a century attributed by Auwers to the principal star is too small to serve as a decisive argument of a common transport through space.

The variations of δ Orionis, although periodic in sixteen days according to Auwers †, are slight and perhaps, like those of many other double stars, intermittent. The *quality* of this star's light changes quite as appreciably as its *quantity*. M. von Konkoly's recent observations appear to establish the alternate conspicuousness and semi-effacement of hydrogen and metallic lines in its spectrum ‡. Simultaneous spectroscopic observations of the companion would be of particular interest as a criterion of physical relationship; but they do not seem yet to have been attempted.

The short-period variable S Monocerotis is a triple star. Its two attendants, respectively of ninth and eleventh magnitudes, are situated at distances from it of 2″·8 and 16″; and the inner and brighter one is dubiously asserted to be in slow circulation. The system has virtually *no* proper motion.

The variability between the fifth and the eighth magnitudes of a star near α Virginis was ascertained by Schmidt in 1866. No attempt has been made to assign a period to fluctuations which proceed, it would seem, with such irregularity as to defy formulæ. Two peculiarities distinguish Y Virginis §. First, its spectrum resembles that of Sirius, and no other Sirian star is known to be subject to extensive light-change of a kind different from that of Algol. Secondly, it is one of Burnham's excessively close doubles. It was divided at Chicago in 1879 into two nearly equal components at a distance of 0″·47. A subsequent observation with the Madison 15½-inch refractor afforded no satisfactory evidence of relative change, either of position or brightness, during the intervening two years. Yet it may confidently be assumed that the observed variability is shared by both members of a system which may prove, when its history has had time to develop and its circumstances to become better known, to be one of the most curiously instructive in the whole heavens.

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* Of many variables in the list, no spectroscopic observations have been published.

† Schönfeld doubts the period, but admits the change, as to which Gould is sceptical.

‡ O'Gyalla Beobachtungen, Bd. viii. Theil i. p. 5. See also Catalogue, No. 410.

§ Otherwise known as Virginis 550=B.A.C. 4531=Ll. 25086.

Among well-known double stars of the Sirian type subject to unmistakable fluctuations of lustre, are γ Virginis, ζ Boötis, π Boötis, 38 Geminorum, α Piscium, θ Serpentis, and β Scorpii. With some hesitation δ Cygni may be added to the list. Colour-changes in the smaller of these two stars are patent. Struve found it of an ashen shade from 1826 to 1833; in 1836 of a notably distinct red[*]. It has since generally appeared blue, but with intervals of greyness and redness.

Light-change in the same star is vouched for by its complete invisibility to Sir William Herschel in 1802 and 1804, as well as by the repeated failures to see it, in 1823 and 1825, under most favourable conditions, of Sir John Herschel and Sir James South. The view that an occultation of one star by the other had occurred in the interim was flatly contradicted by their subsequent movements; and the only hypothesis left available was that of a temporary loss of light in the eighth magnitude component of the pair. M. Dunér, on the other hand, acquired the conviction from seven years of experience that fluctuations in the visibility of this delicate object are due to atmospheric influences alone. The course of its revolutions still remains largely enigmatical. No computed orbit represents them with any approach to accuracy.

With few and doubtful exceptions, revolving double stars vary in concert if they vary at all. The changes of γ Virginis illustrate the mode of procedure in this respect of couples intrinsically equal. They alternate in each component, and can thus be detected only by close attention to the swaying balance of lustre between them. Each may be described as normally of the third magnitude; and each in turn declines by about half a magnitude and recovers within a few days, yet so that the general preponderance, during a cycle of several years, remains to the same star. The existence of this double periodicity was recognized by M. O. Struve, who, however, despaired of successfully investigating it in a latitude where the stars in question never rise more than 30° above the horizon[†].

Their circulation is in the most eccentric of ascertained stellar orbits. The ellipse traversed by γ Virginis in 180 years is in fact proportionately somewhat narrower than the path round the Sun of Encke's comet. The perihelion-passage in 1836 of two bodies subject to such marked fluctuations of mutual influence, and the elder Struve's detection of their alternate inequality during the eighteen preceding years, may not be disconnected facts; and are the less likely to be so that the inequalities developed perhaps by approach promise to become obliterated with progressive separation. The range of variation of each star was in 1851 seven-tenths of a magnitude; it would seem to be at present much more restricted; it may not improbably have diminished to nothing before apastron is reached in 1926.

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[*] "Color comitis egregius" (Mensuræ Micrometricæ. p. 297).

[†] 'Observations de Poulkowa,' vol. ix. p. 122.

The fashion of change of γ Virginis is (at least to some extent) followed by ζ Boötis, Σ 1517, by the close stars OΣ 256, as well as (according to F. G. W. Struve) by the pairs Σ 1122 and Σ 1350. Variations so harmonious obviously imply not only a close physical tie, but a strong constitutional resemblance between the bodies subject to them.

Their course is, it would seem, disturbed in 44 Boötis by the real superiority of one member of the pair. Noted by Herschel as considerably unequal in 1781, they appeared to him perfectly matched in 1787. They had, in the interim, passed periastron. Struve observed, June 16, 1819, a difference between them of two magnitudes, which had sunk to half a magnitude in 1833. Argelander found them precisely equal June 6, 1830, and Dawes perceived, April 27, 1841, a slight advantage on the side of the usually smaller star. During the years 1869–1875 differences of brightness, ranging from 0·4 to 1·3 magnitudes, were recorded at Lund by M. Dunér *. Their photographic magnitudes, as determined at Paris in 1886, were 5·3 and 6. The changes of each component are often simultaneous, though not always in the same direction. Hence their combined variability has never been conspicuous. The stars of 44 Boötis are among those subject to mutual occultations, the plane of their orbit, which is a highly eccentric one, passing nearly through the Sun. Their tints vary from light yellow and sky-blue to white and dull grey.

Concerted light-changes in two adjacent stars obviously afford the strongest presumption of their genuine relationship; but it is not always easy to determine whether the observed fluctuation should be laid to the account of one or both members of a close pair. In a few cases, however, the criterion is of value. Two variable stars in Cygnus †, for example, situated $24''$ apart, may safely be assumed to constitute a system; and their fine coloration in pale red and intense blue is a confirmatory circumstance. A still more striking combination is presented by the variable U Cassiopeiæ and a blue companion with which its ruddy glow at times contrasts splendidly. The principal star fluctuates irregularly from the sixth to below the ninth magnitude; the attendant from the eighth to the tenth. The probability of their being united by a special tie is overwhelming. Agreement in conspicuous variability is an argument for its existence to the full as convincing as the possession of a common proper motion.

A remarkable crimson star known as U Cygni since 1871, when Mr. Knott discovered its variation to the extent of about four magnitudes in a period of 466 days, has also a blue companion at $63''$, which, however, seems to change in colour rather than in light.

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* Lund Observations, 1876, p. 74.

† *h* 1470=Ll. 38428.

Since its azure hue survives unaltered the apparent extinction, with the telescope employed, of the adjacent red luminary, it is certainly original, and not a mere optical effect of contrast. U Cygni is the only star belonging to Secchi's fourth spectral type open to any suspicion of being in systemic connection with a neighbour.

The spectrum of U Puppis is of the solar type. Its variation from 6 to 6·8 magnitude in $14^{d}\ 0^{h}\ 21\frac{1}{2}^{m}$ was detected by Mr. Espin in 1883, and the study of its light-curve derives added interest from the strong probability that it integrates the changes of two close components. The star is the chief member of the wide fixed couple Σ 1097; it was further resolved by Burnham, January 28, 1874 (after being already "elongated" by Dembowski), into two unequal objects at 0″·80. There is no other short-period variable so closely double that "odds beyond arithmetic" might safely be staked on the side of physical connection.

The duplicity of η Geminorum was perceived by Mr. Burnham during a visit to Mount Hamilton in the autumn of 1881. With an eighth magnitude attendant at 0″·80, it forms (he says) "a splendid unequal pair, and likely to prove an interesting system"*. Its revolutions will deserve the more attention that no star showing a banded spectrum has yet given perceptible signs of orbital movement.

As a variable, η Geminorum may be described as an abortive specimen of the Mira class. Its phases, always ill-marked, are at times almost wholly suspended. The share of the companion-star in producing or effacing them has still to be determined: but it is evident that the individual fluctuations of a just separable pair of variables may lead to singular complications in the resultant combined light-change. Agreement in period and phase can seldom occur; hence mutually compensatory fluctuations must occasionally smooth down nearly into a straight line the curve representative of their joint effects. Some of the irregularities of variables may possibly prove to be thus explicable; and it is at any rate clear that no theory of stellar light-change can be satisfactory unless it takes into account the phenomena of relative variability in such objects as U Puppis and η Geminorum. A. M. CLERKE.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The 'Nautical Almanac.'

GENTLEMEN,—

I should be glad if you would allow me to reply to the remarks which have as yet been made on my paper on the 'Nautical Almanac,' in your February number.

As regards the repetition in pages i & ii of each month. Page i is part of the Ephemeris for the Greenwich Meridian. It seems to me very difficult to assign any reason why the Right Ascension of the Sun at Transit at Greenwich should not be given if it be necessary to give the places of the Moon and Planets at their Transits. Every observatory near Greenwich, I imagine, uses these values to compare observations with, if they use the 'Nautical Almanac' at all, and it would have to compute the places for Transit if they were not given in this form. It may be that in a climate so uncertain as England the labour saved in the observatories is far less than that expended in the 'Nautical Almanac' office; but I am disposed to think these Ephemerides could not be given up. Of course, however, page i would go with the Greenwich section, while page ii is quite essential to the general section. On consideration, however, the equation of time, I believe, must stand to complete the general information, but the Sidereal Time at Mean Noon might well be relegated to the computer's part and give the space needed for the additions I suggested.

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* Memoirs R. Astr. Soc. vol. xlvii. p. 204.

As to the Moon Culminating Stars, I would remark that what you find as to the common error of the stars on any night is only what one would expect if their places be taken from a catalogue showing systematic error with reference to the present Greenwich observations. I fail to see, however, what benefit is got, except as to the stars' places, by their observation with the Moon. Perhaps you will allow me to enter on it more fully.

The Moon Culminating Places may be viewed in two ways:—First, they are part of the Ephemeris for Greenwich Transit, and in this point of view the accompanying stars are hardly needed, for the Moon's place is determined from the general instrumental corrections and the time from clock stars. The observations of these stars (if secured) serve to correct their places. In this point of view the space is nearly wasted.

Secondly, they may be used (as they were meant to be used) for the determination of longitude. It was supposed that by comparing the observations at two stations, the Moon being referred to the same stars, the difference of the intervals of transit divided by the hourly motion would give the difference of longitude. Any one looking over the old volumes of the Memoirs or other records of determination of longitude can find instances. Of course the telegraph has entirely superseded this use near Greenwich.

Nor are the Moon culminators more useful away from Greenwich. The use intended was for determining longitude. In the instructions given by the Astronomer Royal for the determination of longitudes in the Boundary Survey of Canada (published in the professional Papers of the Royal Engineers in an early volume about 1850) the observer is told to interpolate the right ascension of the Moon's limb from these data for two longitudes near his estimated value and then to determine his longitude by comparison with these; and somewhat similar instructions were, I understand, issued much more recently to Transit of Venus parties. This process involves the use of differences to the 5th order, and if it were sought to compare an observed right ascension at a known place with the tables by means of these data a similar interpolation would be necessary. Corresponding observations in such cases are but little aid; and if they could be had regularly, the interpolated Right Ascension determined on 5 or 6 nights with different systematic errors for the stars of reference on different nights would not mend matters.

The Ephemeris as given in the 'Connaissance des Temps' gives data from which interpolations for either purpose can readily be made and suitable stars for observations are indicated. For my part I doubt if any result adequate to the labour of computing (save the Greenwich Ephemeris) is gained, but certainly the mode of publishing in the French volume is the more practical. Of course, if the instrument be imperfect, the referring of the Moon's place to adjacent stars will give a better result for its Right Ascension than would otherwise be obtained; that proceeding, however, does not require this Ephemeris. I am, too, of opinion that Equal Altitudes of the Moon and Stars will generally be preferable where the telegraph is not available for longitude.

As regards the Stars. There need be no difficulty, such as you see, in extending the 'Nautical Almanac' list. Practically I conceive that there would be objections to separate lists. The better plan would be to place all the stars in order of right ascension, distinguishing the standard Greenwich stars in some way (I would suggest leaded type) so as to prevent confusion. Nor would I vary the accuracy of the conversion from mean to apparent places, and the extent to which places are shown, which would prevent the less known stars being used for rates on equal terms with the rest and be otherwise undesirable. If the Royal Observatory would observe such stars as are not on its standard list a few times in each period included in a new catalogue we should soon have sufficient places; and failing this, places determined elsewhere could be used to prevent material errors from bringing up for long periods.

While on this subject I would point out that the 'Nautical Almanac' ignores the terms in the nutation which depend on the solar perigee. The influence is small, though not less than that of retained terms; but a comparison of the constants of the 'Nautical Almanac' and 'Connaissance' will show that in February–March and in August–September the difference in date of equal nutation correction is about a whole day.

If I adhere to my opinion that lunar distances are still necessary, it is not because I do not wish it were otherwise. I do not think what I should call *a well-divided* sextant has accidental errors so large as Professor Boss seems to have found according to Mr. Bryant; but, on the other hand, I have met with a very good sextant, by a first-class maker, where the effect of eccentricity was 90″; in fact this last error changes every time the instrument is cleaned or repaired, and the maker may be no way responsible for it. Mr. Bryant's estimate of the probable errors of longitude are therefore no way excessive for the sextant as ordinarily used. One can, however, readily conceive cases where even such errors are better than ignorance, and we have yet to devise a better mode of determining longitude at sea than lunar distances; chronometers are yet far from perfect, and there are cases where a vessel may be away from known points for considerable times. Occultations can manifestly rarely serve the Sailor's purposes. After all, the eccentricity error of a sextant is now determined at Kew for a trifling fee, and it only depends on the navigator to be practically clear of it. Even were the possibility of dispensing with lunar distances clearer than it is, it would be unadvisable to give them up till better ways are known generally, as well as discovered. At present, even for shore use, it would be difficult to meet all arguments for their retention, especially if observers would take the trouble to supply data for refraction and correct for eccentricity.

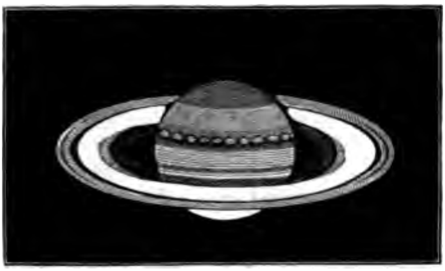
Yours faithfully,

1888, March 20. J. F. TENNANT.

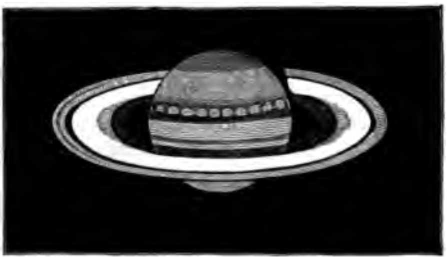
Saturn.

MESSIEURS,—

Comme suite à la communication de M. Elger, je crois vous être agréable en vous adressant les deux dessins ci-inclus de Saturne qui diront, mieux que toute description, ce que montre actuellement mon 8 pouces (réfracteur) de Grubb, avec des grossissements de 250, 420 et 450 fois. Ces dessins sont respectivement du 8 février 1888, à 9 heures, et du 3 mars 1888, à $8^h\ 32^m$. S'ils étaient reproduits bien *exactement, dans leurs moindres détails, par la lithographie,*



1888, févr. $8^d\ 9^h\ 0^m$.



1888, mars $3^d\ 8^h\ 32^m$.

ils donneraient une idée excessivement exacte. Je me bornerai, au surplus, à signaler les points suivants :

(*a*) L'aspect *moutonné* qui règne au dessus de la bande noire principal du globe.

(*b*) La netteté plus grande du bord intérieur de l'anneau obscur sur l'anse oriéntale.

(*c*) La visibilité de la bande de Struve sur l'anse orientale et son état incomplet à l'occident, où elle ne semble occuper que la moitié de l'anse.

(*d*) Les divergences, les contradictions, à ce sujet, entre les divers observateurs semblent indiquer la rotation de l'anneau obscur.

(*e*) Le grand éclat de l'anneau B qui s'obscurcit seulement, et très peu, dans le voisinage de la bande de Struve ; cet état est le même que celui que j'ai signalé pour le mois de mai 1887 (*vide* 'Observatory,' juin 1887, p. 231). Lors de l'*opposition* de 1886–7 l'anneau B offrait un aspect tout différent ; son grand éclat se manifestait seulement dans une zone voisine de la division Cassinienne, le reste de la surface offrant un obscurcissement excessivement marqué sur une grande étendue, jusqu'à la bande de Struve.

(*f*) L'éclat plus grand de l'anneau B doit contribuer à rendre plus difficile qu'à l'opposition de 1886–7, l'observation des détails délicats de l'anneau obscur et de la bande de Struve.

(*g*) La bande d'Encke se voit bien surtout du côté occidental ; le 8 février l'anneau A était visiblement plus clair entre cette bande et la division Cassinienne. La bande d'Encke était manifestement plus près du bord extérieur de l'anneau A.

(*h*) Malgré la netteté moins grande du bord intérieur de l'anneau C à l'occident, je n'ai pas encore revu *avec assurance* de taches sombres dans C, ni de dentelures à son bord interne ; trop souvent ces apparences s'évanouissent pendant les instants trop fugitifs de calme absolu de l'image ; on voit alors paraître un contour net et tranché là où l'on soupçonnait par moments des dentelures ou des ondulations. Il en est de même des dentelures soupçonnées au bord de la division Cassinienne. Souvent l'on a représenté la phase de Vénus avec des ondulations dont l'existence est restée fort problématique et je pense qu'avant de se prononcer sur la réalité de détails aussi délicats, il faut attendre le verdict des instruments les plus puissants, celui de Lick par exemple.

Je suis, Messieurs, votre bien dévoué,

Louvain, 1888, mars 9. F. Terby.

Lunar Eclipses of 1588 *and* 1623.

Gentlemen,—

So much interest has naturally been stirred up by the variation of colour on the Moon's disk in passing totally through the shadow of the Earth that it is of interest to be able to add any more instances to those already known both of black total eclipses and

of very bright total eclipses. From Wendelinus' 'Eclipses Lunares,' published in 1644, we may add the eclipse of 1588 to the first-named class, and that of 1623 to the other class. Wendelinus mentions that "it sometimes happens that the Moon when totally immersed in the umbra so far vanishes out of sight that it cannot be found without an attentive search, a matter that Tycho has left in writing about the eclipse of 1588, which also happened in 1620, and finally in 1642 in the month of April we all perceived it." He goes on to add, "But sometimes it so far retains the light derived from the Sun that you would doubt whether any part of it were eclipsed." He gives as a case of this the eclipse of April 1623 observed by Gassendi, the more remarkable as being one of 9-tenths only. The explanation given is similar to that of Filopanti of Bologna in the 'Observatory,' No. 134. The list of well-authenticated instances of black total eclipses of the Moon will, therefore, be now—A.D. 1588, 1601, 1620 (two), 1642, 1761, 1816, 1884. To these the eclipse of 1110 may doubtless be added. The most remarkable cases of very light eclipses seem to be those in the years 1623, 1703, 1848. Humboldt in 'Kosmos,' vol. iii. part ii., remarks: "While more than half a century ago (29th March, 1801) I was lying at anchor off the island of Baru, not far from Cartagena de Indias, and observing a total lunar eclipse, I was exceedingly struck by seeing how much brighter the reddened disk of the Moon appears in the sky of the tropics than in my northern native land."

Faithfully yours,

S. J. JOHNSON.

Melplash Vicarage, Dorset,
1888, March 19.

The earliest recorded Eclipse.

GENTLEMEN,—

It is well known that Brugsch Bey has maintained, notwithstanding the objections of M. Chabas, that an Egyptian inscription in the reign of Thakelath II. refers to a total eclipse of the Moon. The inscription he translates thus:—

"When now had arrived the 15th year, the month Mesori, the 25th day, under the reign of his father, the lordly Horus, the godlike prince of Thebes, the heavens could not be distinguished, the Moon was eclipsed [*literally*, was horrible] for a sign of the (coming) events in this land; as it also happened, for enemies [*literally*, the children of revolt] invaded with war the southern and northern districts (of Egypt)."

Now the 15th year of this reign would correspond to B.C. 786, when there were two total eclipses of the Moon—on the 18th of March and the 11th of September respectively. But at that time the Egyptian year commenced (with the month Thoth) at a date corresponding to our 8th of March; that year would be completed on the 3rd of the following March, and its last or twelfth month (Mesori) would commence on the 1st of February, so that the 25th of Mesori would correspond to the 26th of February, when no lunar eclipse could have taken place. I conclude, therefore, that the Egyptian inscription relates to some meteorological phenomenon; and indeed it is difficult to see how the expression "the heavens could not be distinguished" can refer to an eclipse.

If this be so, we are probably indebted for the earliest actual record of an eclipse to the Chinese philosopher Confucius, who mentions (in the 'Tchun-tsieou') an eclipse of the Sun which occurred on a date corresponding to the 6th of September, B.C. 776, the first year (by a remarkable coincidence) of the first Olympiad.

This was nearly thirteen years before the solar eclipse which took place in the eighth year of the Assyrian king Asshur-day-an III., on a day corresponding to the 15th June, B.C. 763.

Yours faithfully,

Blackheath, 1888, Mar. 7. W. T. LYNN.

The Visibility of the Unilluminated Part of Venus.

GENTLEMEN,—

In connection with Mr. Lynn's interesting letter on this subject, I would say that I have not unfrequently seen the secondary light of Venus some time after sunset, notably during the spring of 1873. On reference to my observation-book, I find that it was strongly suspected at $6^h\ 30^m$ on March 16 of that year, when the illuminated portion of the disk was about equal to ·380, and was afterwards undoubtedly observed on March 20, March 22, March 24, and March 31, at times varying from 50 min. to 90 min. after sunset, while on other evenings, to all appearance equally favourable, no trace of it could be detected. When visible, it seemed, if possible, to become more obvious as darkness increased. Its tone always appeared greyish and was evenly distributed.

I used powers of 115 and 200 on an excellent achromatic by Cooke, and may add that it was also very satisfactorily seen with the aid of the plane used for solar observation.

Yours faithfully,

Kempston, Beds, 1888, March 19. THOS. GWYN ELGER.

An Andromede Meteorite.

GENTLEMEN,—

In a letter which appeared in 'The Observatory,' No. 103, I expressed a hope that some astronomer would succeed in capturing a Leonid or an Andromede, so as to set at rest the question whether meteors or meteorites were of the same nature and origin. I was not aware until very recently that this had actually been done on the occasion in question, and I think the information will be new to a good many of your readers. A full account of this meteorite will be found in the 'Century Magazine' for August 1887. It fell about 9 o'clock on the evening of the 27th of November, 1885, at Mazapil in Mexico, at which place a brilliant shower of Andromedes was observed on the same night. It was seen by a ranchman, from whom Professor

Bonilla, of Zacatecas Observatory, obtained it five days afterwards, and when first seen he described it as "a ball of light." It proved to be a piece of meteoric iron, with the usual mixture of cobalt and nickel. We have thus, in all probability, secured a genuine fragment of Biela's comet.

Yours truly,
Dublin, 1888, March 6. W. H. MONCK.

The R.A.S. Medal.

GENTLEMEN,—

In the account you gave in your last number of the meeting of the Royal Astronomical Society, I notice that the reporter has made one or two mistakes and important omissions in my speech. It was Mon. Hermann Goldschmidt, not "Mr. Goldsmith," of whom I spoke, and who received the Gold Medal for the discovery of 13, not 12, Minor Planets; also I said that this "was the largest number found by any one man, *at that time.*" The words in italics being an important omission, seeing that Palisa had, up to September last, discovered 60 of these small planets since 18th March, 1874. Very much was thought of Goldschmidt's achievement at that time, for only ten years before Gasparis had received the Gold Medal for the discovery of only *three* minor planets, and in 1853 Hind for discovering *eight.*

I was present at the meeting in 1861, and remember so well Admiral Manners receiving the Gold Medal for Mon. Goldschmidt, whom the Admiral said was only a copier of pictures, and possessed a small and not powerful telescope.

It was, I think, one cause of the Emperor Napoleon's gratification that the Gold Medal had been so *impartially* bestowed upon one of his subjects in comparatively humble life. This impartiality in adjudging the Gold Medal doubtless renders it so valuable in the eyes of all the world, and is a strong reason for its continuation.

Yours faithfully,
Holm Lea, Guildford, SAMUEL KINNS.
1888, March 6.

A fine Meteor.

GENTLEMEN,—

A letter from Paris tells me that a splendid meteor was seen on the 3rd of this month, travelling N.E. from S.W. My informant, who was walking in the Champs Elysées and saw it about 9.55 P.M., states that it had "a long violet-blue tail, edged with green-blue, and left a long streak of light after it." The sky was not clear enough to see by what stars it passed. Perhaps some other correspondent may send you a further account.

Yours truly,
Ashdown Cottage, HENRY J. SLACK.
Forest Row, Sussex, 1888, March 6.

OBSERVATORIES.

Publicazione del Real Osservatorio di Palermo.—This volume contains the observations made at the Observatory at Palermo, under the direction of Prof. G. Cacciatore, during the years 1883-4-5. The great bulk of the volume is occupied by the usual meteorological observations, with monthly and yearly means. In the astronomical portion are numerous physical observations of the Sun, tables of spots, faculæ, and prominences observed during 1883 and 1884. In 1883 observations were made on 313, and in 1884 on 283 days. The spots in south latitude numbered twice those north of the equator. Then follow physical observations of Jupiter, including transits of the great Red Spot during the apposition of 1883–4, and a complete account of the Andromeda meteor-shower of Nov. 27, 1885, with the calculation of an orbit the coincidence of which with that of Biela's Comet is remarkably close. There are a few observations of small planets and comets, and a redetermination of the latitude of the observatory. The method employed is by observations of stars on each side the zenith. The results are not very accordant, there being a range of 7″·6 between the individual observations, and a range of 4″·6 between the mean of one night's work and another. These discordances are in a great measure due to bad graduation of the circle; and until these errors are investigated and allowed for, the resulting latitude 38° 6′ 45″·59 N. (obtained by giving each observation the same weight) cannot be considered as final.

We are very glad to hear that another fine observatory is to be added to the not too numerous list of private observatories in England. Sir Henry Thompson, the eminent surgeon, has had erected by Messrs. Cooke, of York, at his residence Hurstside, West Molesey, a fine equatoreal telescope of twelve inches aperture, under a dome of their make large enough to take a still larger telescope, if at any time this is found to be necessary. A photographic objective is to be mounted on the equatoreal, and with a prism in front of the object-glass will be available for photographing stellar spectra. We understand that the equatoreal is almost complete, and as soon as the whole is ready work is to be begun.

PUBLICATIONS.

The Constant of Precession and the Motion of the Solar System in Space *.—Herr Ludwig Struve has carefully compared recent Pulkowa catalogues with Bradley's observations as re-reduced by Auwers, thus obtaining an interval of a century for the determination of proper motions and precession. He first reduced all the stars to the epoch 1810 with Otto Struve's precession constant (determined in 1841) and deduced a series of preliminary

* Mém. de l'Académie de St. Pétersbourg, vii. série, t. xxxv. 3.

proper motions, affected by error in precession constant, and apparent displacement due to the Sun's motion in space. The number of unknowns involved is thus five; but different independent variables can be chosen, resulting in different forms for the equations. The author selected the equations used by Sir George Airy (Mem. R. A. S. vol. xxviii.), who took the three rectangular components of the Sun's velocity as variables. He divided the 2500 stars into 120 groups, and since each group gives two equations—one in R.A. and one in N.P.D.—he obtained 240 equations to be solved by least squares for the determination of the 5 unknowns. He also represents in his equations Herschel's hypothesis as to a general rotation of the stellar universe; but the solution shows that this hypothesis should be rejected. It was necessary to assume some connection between the magnitude of a star and its distance; and he adopted the following relative scale, referred to stars of the 6th magnitude as standard:—

Mag.	Dist.	Mag.	Dist.
1	0·13	5	0·70
2	0·23	6	1·00
3	0·36	7	1·49
4	0·51	8	2·25

The following table shows the deduced value of the luni-solar precession compared with that of previous calculators:—

	″		″
Bessel	50·3635	Bolte	50·3584
O. Struve	50·3798	Bolte	50·3570
Nyrén	50·3269	Bolte	50·3621
Dreyer	50·3820	L. Struve	50·3514

The following is a comparison of the values obtained for the angular coordinates of the point to which the solar system is moving:—

	A. °	D. °	Epoch.	No. of Stars.
W. Herschel	260·6	+26·3	....	....
W. Herschel	245·9	+40·4	....	....
Gauss	259·2	+30·8	....	....
Argelander	259·9	+32·5	1792·5	390
Lundahl	252·5	+14·4	1792·5	147
O. Struve	261·5	+37·6	1790	392
Galloway	260·1	+34·4	1790	78
Mädler	261·6	+39·9	1800	2163
Airy	261·5	+24·7	1800	113
Dunkin	263·7	+25·0	1800	1167
Gylden	273·9	....	1800?	?
Gylden	260·5	....	1800	?
L. de Ball	269·0	+23·2	1860	67
Rancken	284·6	+31·9	1855?	106
Bischof	285·2	+48·5	1855	480
Ubaghs	262·4	+26·6	1810?	464
L. Struve	273·3	+27·3	1805	2509

And the following determinations from the Cape Catalogue should be added:—

	A.	D.		No. of Stars.
	°	°		
Plummer ……	270·1	+20·3	….	274
Plummer ……	276·1	+26·5	….	274

As regards the magnitude of this motion the determinations are less numerous; and omitting Airy's, which was determined from stars of large proper motion and is therefore too large, we have the three values:

O. Struve ..	4″·31
Dunkin ….	5 ·22
L. Struve ..	4 ·36

for the angular motion as seen from a star of the sixth magnitude; but Ubaghs and Brünnow both obtain much smaller values of this quantity; and the spectroscopic determinations of motion in the line of sight give a much larger value. This quantity cannot therefore be regarded as satisfactorily determined.

At the end of the paper the author treats of the planetary precession and the secular variation; and gives a list of stars whose proper motions as found by him differ from those deduced by Auwers from Greenwich and Berlin observations.

Rotation of the Sun.—Mr. Henry Crew contributes to the 'American Journal of Science' for February an extremely interesting paper on the period of rotation of the Sun, as determined with the spectroscope by measures for motion of the limbs in the line of sight. He used a large grating-spectroscope, the grating (by Rowland) being 4 inches in length, and the collimator and telescope each of 6½ in. aperture and about 7 ft. focal length. The spectrum of the fourth order was used. The slit was placed tangential to the Sun's limb, alternately at opposite extremities of a diameter, and the double-displacement of corresponding lines measured by a cross-wire micrometer. The extremities of the diameter were varied in solar latitude from 0° to 45°, and an extensive series of experiments was made on at least nine different lines in the spectrum. If a good result can be got by multiplying observations, therefore, Mr. Crew's mean result of

2·437 ± ·024 miles per second

for the mean equatorial velocity (corresponding to a true rotation-period of 25·88 days) should be a good one. It may be remarked, however, that in a paper read at the Manchester meeting of the British Association in 1887 (see Report, p. 583), Mr. Crew gave the result obtained from observations between latitudes 23° and 33°,

2·827 ± ·02 miles per second,

corresponding to a period of 22¼ days. This is doubtless deduced from a less extended series of experiments; but unless the probable error is erroneous, the discordance between the two results is too serious to be accidental. The present paper, however, gives ample material for discussion of this discordance. Mr. Crew points out that there is a great discrepancy between the results deduced from different lines of the spectrum, as the following table shows:—

Line.	Eq. Vel.	Line.	Eq. Vel.
E_2	2·289	D_1	2·420
1474	2·291	5173·8	2·590
E_1	2·302	5171·7	2·608
D_2	2·320	5166·3	2·670

None of these velocities, however, are so high as the preliminary result, 2·827.

There is apparently another systematic error affecting the observations, depending on the date at which they were made, which Mr. Crew has not noticed in his paper. If we take his results on p. 156, and first correct for the line observed, from the above table, and then group the observations under dates, we get the following differences from the mean value:—

Feb. 28	+·066	April 8	+·142
Mar. 8	−·025	July 12, 14	−·063
„ 11	+·105	„ 15, 16	−·183
„ 15, 16	−·025	„ 18	−·093
„ 30	+·193	„ 30	−·122

It certainly seems as though there is here some uncorrected systematic error, though it is difficult to understand how such can have escaped the careful vigilance which Mr. Crew has obviously exercised. Refraction would of course have a small effect in altering the apparent shape of the Sun to an ellipse, and its effect can be readily calculated, but appears to be much too small to have anything to do with such numbers as the above. There is probably some unequal heating of the different parts of the apparatus, especially likely to occur in such experiments, and especially difficult to detect and measure. But until such corrections can be dealt with more satisfactorily, we must hesitate to accept the result which the author deduces for the variation of velocity with latitude. According to his uncorrected table, the angular velocity increases with the latitude; but it will be noticed that a great number of his measures in high latitudes were made in March, when the velocity seems to be systematically too great. If we make the very rough assumption that the observations in March are systematically erroneous with respect to those made in July, and thus obtain a mean difference of −·158 miles per second to apply to the March observations, the following corrections are applicable to groups of 10° of latitude:—

Latitude.	Mean Eq. Vel.	Correction.	Corr. Value.
From 0°–10°	2·309	−·053	2·256
„ 10–20	2·360	−·077	2·283
„ 20–30	2·668	−·145	2·523
Above 30	2·480	−·069	2·411

The discrepancy seems to be therefore due in some measure to this effect of the Sun's altitude on the measures. We hope that

Mr. Crew will prosecute his inquiries further towards the elucidation of these two disturbing causes, especially the former; for the influence of the particular line measured on the result may easily have an important physical significance.

TEMPEL'S FIRST PERIODIC COMET.—We have received a memoir by M. Raoul Gautier, of Geneva, on the above comet (1867 II.), treating especially of the returns in 1873 and 1879. The comet was discovered in 1867 and observed carefully in that year and at the reappearances in 1873 and 1879, but was not seen in 1885. Between 1867 and 1873, however, the orbit was greatly disturbed by the planet Jupiter, whose orbit is almost a tangent to that of the comet; and the determination of the mean motion from observations in 1867 and 1873 is thus a matter of some difficulty. But between 1873 and 1879 Jupiter was never near the comet, and the observations in these years thus give an accurate orbit. M. Gautier has corrected his first orbit for the influence of all the planets (except Neptune) during the period 1873–1879, and obtained a still more accurate result. The value of the period is 6 years all but 2·68 days. M. Gautier hopes, by carrying back this orbit from 1873 and allowing for the action of Jupiter, to get better elements for the epoch 1867.

THE AIRLESS AND WATERLESS CONDITION OF THE MOON.—We notice in the 'Proceedings of the Liverpool Geological Society,' 1887–88, an interesting paper by the Rev. F. F. Grensted, entitled "A Theory to account for the Airless and Waterless Condition of the Moon." The author reasons that the high mean density of the Earth as compared with that of the crust is due to the fact that the metals exist in a state of greater purity in the interior, while the crust is composed of comparatively light oxides; that oxidation has proceeded to a depth of something like 700 miles at most, or one sixth of the radius. Similarly he calculates from the known size and density of the Moon that her oxidized crust extends to a depth of 478 miles, or nearly half the radius.

"Next follows the main point of the theory. If it be true that both Moon and Earth have a metallic nucleus with an oxidized crust, why has the one air and water, the other not? It is simply a question of *size*. The Moon has a larger surface than the Earth in proportion to her bulk, and therefore more of her mass was oxidized. The air and water were *chemically* not *mechanically* absorbed when her surface was above a red heat. It needed a greater proportion of slag to protect the metallic nucleus; the oxygen &c., which would have made air and water, was thus used up; we, on the other hand, have free oxygen left." The author calculates that the oxidation of pure air to a depth of half a mile further would use up all the oxygen of our atmosphere and ocean—indeed the former would only suffice for a depth of about half a yard; and he makes a further speculation that the luminous rays extending from Tycho and Copernicus &c. at the time of full moon may be dykes of pure metal, which the comparatively small quantity of oxygen available on the Moon has left untouched.

My Telescope (by a Quekett Club-Man) can be read right through in twenty-one minutes, and contains, perhaps, as much information as can be well conveyed in that time. Further the information is generally correct, though of a most elementary character. But there are numerous faults in the book. The word "infinite" is used recklessly. "Mars is often much nearer (than his mean distance) and frequently infinitely further off." "Jupiter's satellites are infinitely small as compared with him;" and so on. "Perpendicular" is used for "vertical." We are told that when an object is found with an equatoreal, it can be kept in view by one motion only of the instrument, *the rotation of the earth itself supplying the other movement*!

We do not quite know in what language the book is written; we hope it is not American.

Anuario del Observatorio de la Plata. Annuario publicado pelo Imperial Observatorio de Rio de Janeiro.—We have received the volume for 1888 of the former of these publications, and those for the years 1885–87 of the latter, 1885 being the first year of publication. They both follow the lines of the 'Annuaire du Bureau des Longitudes' and others; the nearest approximation to which in England is Whitaker's Almanac. It is of course convenient to have such books of reference in different languages, but it seems almost a pity that so much valuable time is spent over duplicate calculations. We are reminded how rapidly the number of minor planets is increasing by finding that their elements occupy nearly 30 pages in each of these volumes.

NOTES.

The International Astronomical Congress. — Admiral Mouchez gives in the 'Annuaire' for 1888 of the Bureau des Longitudes, a very interesting notice of the Congress held in Paris last April. This is a sequel to the still more interesting note published the year before, and shows how much of the programme then proposed has been adopted by the Congress, together with some suggestions how the conclusions so lately arrived at might, in the opinion of Admiral Mouchez, be advantageously varied.

The results of the Congress have already been published, and are known to our readers. Obtained as they were with practical unanimity, they have been since accepted as being a satisfactory basis on which the great work the Congress met to discuss might be undertaken with the certainty of success.

While certain main lines were laid down absolutely, a great deal between these lines was left to be varied as further experience might show to be desirable; and it seems very important that the limits marked out by the Congress should be adhered to in the strictest possible manner. Admiral Mouchez, however, suggests that the limit fixed by the Congress as the magnitude of the stars of the chart should be extended from the 14th to the 15th or even further, and discusses what effect such a change would have in the time to be taken for the whole work.

Every one will agree with Admiral Mouchez that it would be a grander thing to get another magnitude, and the Congress no doubt felt this; but the absolute necessity of keeping the scheme within reasonable limits, both as to time and money, had its effect in determining the magnitude that was then fixed, and this magnitude was arrived at more as representing what could be got by a certain exposure than as an arbitrary standard to be reached at all costs.

In this notice, which every one interested in the subject should read, Admiral Mouchez, after considering the number of hours during which work can be done in the year, comes to the conclusion that, with the exposure necessary to get the 14th magnitude, one or two years will suffice to do the work, and that four or five years would suffice to get another magnitude on the plates.

If the work can be done in such a time, it will be a source of intense satisfaction to all concerned in the production of the chart, but already opinions differ as to the time it will actually take. If the assumption that the work can be done in four or five years is correct, and it is done in this time, it will make the first photographic chart of the heavens such a success that another on a much greater scale could be at once undertaken. It might be that it would be better to make some sacrifice now in order to obtain the still greater results that would then assuredly follow.—A. A. C.

THE following particulars have been taken from the 'Richmond Despatch,' U.S.A., Feb. 19, 1888:—

Miss Maria Mitchell has just resigned her Professorship of Astronomy in Vassar College, New York. She has held that position for 22 years, and only now gives it up because of the increasing infirmities of old age, having attained the threescore years and ten. She belonged to an old Quaker family; her father, William Mitchell, was a bank-cashier and an ardent amateur astronomer. As early as the age of eleven she watched the chronometer and marked the time while her father observed through a telescope the beginning and end of a lunar eclipse. At nineteen she became librarian of the Kantucket Athenæum, employing her leisure in astronomical observations. In 1847 she discovered a comet on Oct. 1 (two days before it was seen by Rev. W. R. Dawes at Cranbrook). This was the means of introducing the young lady astronomer to the astronomers of the old world. The King of Denmark gave her a gold medal in recognition of her discovery. She has the credit of discovering seven other comets. Soon after the discovery of the first comet she visited the old country; and it would be of interest to the readers of the 'Observatory' to learn that she was the guest during her stay in England of Sir John Herschel and Sir George Airy, then Astronomer Royal. She was the first Professor of Astronomy in Vassar College. Three institutions have conferred upon her the degree of LL.D.

TOTAL LUNAR ECLIPSE OF 1888, JANUARY 28.—Through the kindness of Dr. Lindemann, of the Pulkowa Observatory, we are enabled to give a complete list of the returns made up to March 12 by the various observatories to which lists of stars to be occulted had been sent, and at which observations have been secured.

	No. of Obs.		No. of Obs.
Algiers		Madrid	20
Amherst (U.S.)	7	Marseilles	39
Armagh	9	Montreal	6
Belgrave	3	Moscow	15
Bilk	8	Neuchâtel	6
Birkdale		Nice	24
Bordeaux	21	Padua	4
Bothkamp	30	Paris	12
Clinton (U.S.)	3	Park Gardens (Bath)	24
Colebyfield	5	Princeton (U.S.)	8
Collegio Romano	5	Pulkowa	50
Copenhagen	25	San Fernando	10
Dublin	35	Stonyhurst	17
Fernhill	14	St. Petersburg	2
Geneva	23	Strasburg	10
Glasgow (Engl.)	13	Taschkent	21
Harvard College	23	Toulouse	13
Harrow	5	Turin	32
Helsingfors		Utrecht	15
Kiel	36	Washington	11
Kis Kartal	2	West Point (U.S.)	2
Liverpool	11	Wilhelmshaven	2
	278		343

343
278
621

NEW TRANSIT-EYEPIECE FOR THE SUN.—In the 'Comptes Rendus,' cvi. No. 9, M. Gruey discoursed at some length on the difficulties of observing all four limbs of the Sun at the same transit without undue haste; and proposes to add two eyepieces, fixed at about 15′ north and south respectively of the ordinary transit-eyepiece, and each furnished with a Z.D. micrometer, so that the Z.D. limbs may be observed without unclamping the instrument. In several ways this would be a convenience; but it seems to us rather a ponderous method of overcoming somewhat over-estimated difficulties.

COMET *a*, 1888 (SAWERTHAL).—This comet was noticed, without previous knowledge of its existence, by Commander H. Moore Ingram, of ss. 'Kangra,' in long. $2^h\ 33^m$ E., lat. 21° 0′ N., on Mar. $14^d\ 14^h\ 4^m$ G.M.T. He notes the position as 15° due south (vertically below?) β Capricorni. The size was apparently that of a star of 1st magnitude, but of a faint white appearance; the tail was 5° or 6° long and about 2½° wide at the widest part, very faint.

On March $13^d\ 16^h\ 32^m\ 25^s$ G.M.T., the position was

R.A. $21^h\ 2^m\ 30^s{\cdot}5$, N.P.D. $110°\ 59'\ 20''{\cdot}8$,

which gives a correction of $-1^m\ 2^s{\cdot}6$: $-43'{\cdot}6$ to the following ephemeris* for Berlin Midnight:—

	R.A.			Decl.			
1888.	h	m	s	°	′	Log r.	Log Δ.
Apr. 3..	22	13	45	+ 7	35·9	9·8821	0·0728
7..		26	36	11	39·3	9·9035	0·0934
11..		39	8	15	18·4	9·9267	0·1134
15..	22	51	21	18	35·7	9·9507	0·1328
19..	23	3	11	+21	33·5	9·9749	0·1513

THE Front Court of the Greenwich Observatory has of late been scarcely recognizable, owing to the accumulation of bricks and rubbish, which the builder seems to delight in. The Computing rooms are being extended, and a new storey, with photoheliograph dome, added. It will be several weeks longer before these operations are complete. The Computers are meanwhile lodged in the Octagon room, which has not presented such a busy appearance since the Transit of Venus (1874) reductions.

THE Fellows of the Royal Astronomical Society, particularly those on the Council, will learn with pleasure that it has been decided to fit up the official rooms with the electric light, the current being obtained from the London Electric Supply Corporation, of New Bond Street.

THE friends of Captain Noble will heartily sympathize with him in the loss and great inconvenience he has suffered through his residence at Uckfield having been entirely destroyed by fire. We understand that the Observatory has not been injured.

SOME excellent articles have lately appeared in 'Engineering,' on the interesting exhibit of telescopes and instruments made by Sir Howard Grubb, at the Jubilee Exhibition, Manchester.

WE are requested to announce that Mr. Edmund Neison, of the Natal Observatory, has recently changed his name to that of Edmund Neville Nevill.

WE see in the 'Journal of the Society of Arts,' that on the 18th inst. Sir Howard Grubb gives a lecture on "Telescopes for Stellar Photography."

MINOR PLANET (No. 273) was discovered by Herr Palisa on March 14.

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* Astr. Nach. No. 2830.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 136. MAY. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, April 13, 1888.

W. H. M. Christie, M.A., F.R.S. (Astronomer Royal),
President, in the Chair.

Secretaries: E. B. Knobel and Col. Tupman.

The Minutes of the previous Meeting were read and confirmed.

Col. Tupman. 74 presents have been received since the last meeting. There is nothing calling for very special mention.

A vote of thanks to the donors was passed.

Mr. Knobel read a communication from *Dr. Gill* on the observation of Dr. Döllen's list of stars observed at the Royal Observatory, Cape of Good Hope, during the total eclipse of the Moon 28th Jan., 1888. In accordance with Dr. Döllen's request, preparations were made for observing, during the total eclipse of the Moon on 28th Jan., the occultations of stars whose time of disappearance and reappearance had been computed by him. It was to be regretted that his letter containing the request in question did not reach the Cape till about a week before the eclipse. Otherwise it might have been possible to comply with his further request to establish a temporary observatory twenty or thirty miles distant, so that the same occultations might have been observed at slightly different parts of the limb. Ten disappearances and seven reappearances were observed by Mr. Nevill, the particulars of which would doubtless be communicated to the Society. The eclipse of 1888 was remarkable for the brilliancy of the eclipsed disk as compared with that of 1884, and this brilliancy rendered it impossible, at least with comparatively small instruments, to observe the disappearance or reappearance of the faintest stars on the list. With a 5-in. telescope in several instances the light from the limb of the Moon produced apparent occultation too soon and apparent reappearance too late. The instruments with which observations were made were: Dr. Gill 7¼ in., Finlay 7 in., Maclure 5 in., and Cox 6 in. Almost all the observations were regarded as very good.

Mr. Turner. It will be a curious fact about the observations of this eclipse and that of 1884, that, as a general rule, the reappearances seem to outnumber the disappearances. It is not a thing which we would expect. It is easier to convince one's self of the reappearances than of the disappearances.

A vote of thanks was passed to Dr. Gill for his paper.

Mr. Knobel read a communication from *Professor Holden*, Director of the Lick Observatory, California, on the probable meteorological conditions attendant on the total eclipse of the Sun observable on Jan. 1st, 1889, in California. Prof. Holden said that in order to assist intending observers to form their programmes of observation he had requested the authorities of the United States Signal Service at Washington and San Francisco to furnish him with the records of cloudiness near the line of total eclipse for the last fifteen days of December and the first fifteen days of January. Prof. Holden pointed out that while the coast range of mountains was the region of rainfall, the valleys of the interior were on an average far more clear. January was a particularly unfortunate time of year for astronomical observation, since it fell in the middle of the wet season, when rainy periods of a week or more were experienced all over the northern part of California. Prof. Holden also dealt with the question of transport by means of railways to the scene of observation. There were good waggon-roads leading from the stations to the points likely to be chosen as observing stations. There would be no difficulty in procuring suitable transport waggons at the stations. With regard to instruments, intending observers might bring them as personal luggage to New York; there they would have to be submitted to rigid Custom House inspection, but admitted free of duty on production of the special permit from the Secretary to the Treasury at Washington. He understood that Congress would be asked to vote a sum of money for the observation of this eclipse. Instruments should be shipped by express from New York to San Francisco, whence they might be "expressed" to almost any point in California. It was hoped that none of the foreign visitors would leave California without paying a visit to the Lick Observatory, but appointments for the visits should be made several days in advance. Prof. Holden regretted that the opportunity of such visits fell in the most unfavourable season, but he would hope that the weather would permit of some idea being formed of the instruments and equipments. At any rate he was sure he could promise to foreign guests a magnificent mountain view and a hearty welcome. Prof. Holden's paper was accompanied by a map, showing various stations on the railways and some data as to the topographical conditions.

Mr. Turner. If I might again claim the indulgence of the meeting in making a remark I should like to say what a very excellent paper this is for intending observers. When thinking about going to Russia to observe the eclipse last year this was exactly the kind of information of which the want was felt. Prof. Holden seems to have anticipated in the most accurate manner the wants of observers who are going to America next year. If there had not been a really capable man in Prof. Struve last year, who supplied that information on the spot. I fancy that the forty or fifty people who assembled rather at haphazard just before the eclipse would have met with a good many difficulties besides the ones which they encountered in the shape of clouds. As a matter of fact the way was smoothed most carefully, and I can only say that from the paper we have before us Prof. Holden seems to be ready and willing to undertake just the same kind of responsibility next January, and the paper we have before us may be taken as representing that most amiable intention. I am quite sure if any one here or elsewhere does go over to America he will find much more fully than words can express what is the value of such assistance in eclipse observing.

The President. I need hardly ask you to return thanks to Prof. Holden for his very valuable paper. I am sure intending observers of this eclipse in California are most fortunate in having such a competent astronomer as Prof. Holden on the spot to furnish them with all these important data which are so necessary for the proper organization of any eclipse expedition. I will now call upon Sir Howard Grubb to give us a description of a new form of driving-clock which he has brought to the meeting to exhibit to-night.

Sir Howard Grubb. Mr. President and Gentlemen, in a paper of mine, read before this Society last spring, I mentioned that it appeared to me likely, so far as one could see at that time, that the best system of producing an accurate clock, one suitable for photographic work, would be a system which was a combination of Dr. Gill's as applied to Lord Crawford's 15-inch equatoreal, and mine as applied to Mr. Roberts's equatoreal. You will remember, probably, the description of the instrument at Lord Crawford's. That clock was of Dr. Gill's design and a portion of that apparatus I thought was probably the best that could be done in that respect, but I thought a combination of that and of a portion of mine would probably form the best that could be made. I worked out this and I now have the opportunity of showing you the result. I have carried out exactly what I intended. One portion, what I call the detective part of the apparatus, is due to Dr. Gill, but I retain what I call the correctors—the accelerator and retarder—of my own form for reasons I will state. I may mention that this identical clock is intended for one of the new photographic instruments (Mexico), and I had better mention that the actual position of the parts in the telescope will not be as it is here. It is merely erected here in a temporary manner in order to show the working. In the first place, the governor of the clock is a very heavy governor, it weighs about 25 lbs., and its normal rate of speed would be about 90 revolutions a minute, but there are springs which supplement gravity and increase the rate of the governor to 135 per minute. This gives an enormous amount of "fly-wheel" power to the governor, and enables it to get over small obstacles, having something like 10,000 feet pounds a minute. On the shaft of the governor is a screw, and that screw gears directly into a toothed wheel upon the counter shaft of the endless screw which works in the sector. By adopting this plan all the wheel-work and all the gearing that is behind the governor merely acts in transmitting the power from the barrel to the governor. Any error in these wheels has nothing to do with the accuracy of the motion of the screw. So you at once eliminate all the errors in these wheels. The next point that I will call your attention to is one that cropped up often during the designing. To relieve the friction of this very heavy governor I support most of the weight (about 23 lbs.) on a set of bicycle balls rolling between two hardened steel disks. By this means I reduce the friction to about one third, and there are only 2 lbs. out of the 25 lbs. bearing upon the agate plate under the "toe" of the governor. This not only reduces the friction considerably but it also prevents the danger of biting or tearing. I do not think there is anything more with regard to the clock that I have to mention except that it is made more substantial and stronger than is usually the case. I will now proceed to speak about the control. The first part of the control is of course the pendulum for controlling. This clock can be controlled by the pendulum of any ordinary sidereal clock, but I have thought it well to employ a special pendulum because observers do not like their sidereal clocks tampered with. Also in a paper last year I pointed out that there would be an advantage sometimes in giving a slight losing rate to the clock in order that it may possibly allow to some extent for refraction. Of course with the sidereal clock in an Observatory you cannot do that, because whatever clock you use becomes the governor, and if that be the standard clock of the Observatory you cannot tamper with it, but by employing an independent pendulum you can do what you like. I think most of you know there are two other forms of control which within the last four or five years I devised, and I wish to touch upon these for a moment to mention that this form has been devised *not* from any failure on the part of others, but to meet the extra requirements which are every day cropping up. The first one I devised many years ago does its work well so far as it goes, as may be judged from this chronograph sheet from the Dunsink Observatory. Then there is a second system, that which I adapted to Mr. Isaac Roberts's telescope, and which I also adapted to Prof. Pritchard's telescope. I am sorry Prof. Pritchard is not here to-night to mention the result of that control, but he authorizes me to say that he is perfectly satisfied of the feasibility of driving his telescope sufficiently accurately without that continual observation by the eye which is necessary in ordinary clocks. I mention this to show it was not from any failure on the part of these former controls, but simply because astronomers every day ask for more and more delicacy in their clock-movements. I think it will be found that for all practical purposes this clock will act sufficiently well for photographic work.

(Sir Howard Grubb then went on to describe minutely the working of the pendulum and of the clock.)

The President. I will now invite any remarks on this very important communication. I think an observer without a good driving-clock is completely at sea in making correct astronomical observations. Therefore this question of the driving-clock is to my mind a most important one in modern extra-meridional astronomy. I think it will be convenient to discuss the question of the clock first, and then to consider this ingenious slow motion afterwards.

Mr. Common. I suppose if anybody were to set about obtaining some mechanical substitute for an observer, he could not but call electricity in to help him; and it struck me when listening to Sir Howard Grubb's remarks in explanation of the working of this clock that, if we had some means of using the image of the star itself to regulate the motion, we should get a perfect clock. If we could find a substance that would vary in its conductivity sufficiently through the impact of light, as selenium does to some slight amount, and we placed the image of the star between two bars in such a way that if it touched one bar it would retard the clock and if it touched the other bar it would accelerate it, we would then have an idealy perfect clock. There is not the slightest doubt that having this is the nearest approach to a good clock it is possible to conceive. The principle is everything that one could wish for. The question of how delicate it shall be is a mechanical matter. Electricity will never fail a man up to any point he could possibly wish: it is capable of any delicacy that his senses could possibly perceive. I believe I am right in that; so I say we would have here the nearest approach to a perfect clock that it is possible to have. Of course the question of how the clock is to be used comes in. Sir Howard Grubb has gone on the principle that, in order to retain the image of the star upon the same point of the photographic plate, it is necessary to move the whole of the optical apparatus and all that is carrying it the necessary amount to effect that object, and correct in that way the errors due to the shifting of any part, of refraction, or anything that may occur during exposure.

As most of the Fellows may know who have thought about it at all, there are two ways of getting the image on the plate persistently on one point—you may move the instrument or you may move the plate in order to correct the errors mentioned. In 1882 I thought the easiest plan would be to move the plate as Draper and De la Rue had proposed some years ago, thinking that, if you could only obtain by some mechanical means a uniform motion for the ordinary movement of the telescope that should not in its total error exceed the range with which you could follow the star with your slow motion, it would be better to move the plate than the telescope. The course I follow is to move the plate to the right point, and correct all the errors by the movement of the plate. You can do this within certain limits without bringing in any optical disturbance. If it was a question of moving the whole plate one or two minutes of arc from the true axis, it would not matter much; but it would produce distortion if you had to move it, say, four or five minutes of arc from the centre; but in practice the small errors can be brought down to a very small amount. There is a difference of opinion as to which is the best; I think in a large instrument where there is an enormous mass to be moved it would be better, perhaps, to correct by moving the plate in preference to moving the telescope; but in cases of well-balanced equatoreals, such as are contemplated in the photographic survey, it would be far better to move the telescope than the plate. I wish to express my hearty appreciation of the efforts Sir Howard Grubb has made in improving the working of the driving-clock, because, although it is perhaps not properly appreciated, the whole success of such a work as the charting of the heavens actually comes to this, that it largely depends upon the perfection of the clock. Therefore I am sure that astronomers ought to be greatly indebted to Sir Howard Grubb for the care and skill he has brought to bear in bringing the driving-clock into such a state of perfection.

Mr. Crossley. Mr. President, Mr. Common limits the application of a controlled and corrected clock like this to comparatively small telescopes. I do not see why it should not be applied to telescopes of any size whatever. I must confess I think it would be infinitely better to do so than to attempt moving the plate for the purpose of correction. There is always a difficulty in moving the plate the necessary amount and in the right direction, whereas if you only move the telescope you have your telescope much more accurately handled and with the polar axis in proper position. I think it is much better to apply a system of this kind to the telescope, whatever the size of the instrument may be.

The President. I must say this seems to me to be a very admirable contrivance for securing perfect accuracy in the movement of a driving-clock, and as far as I can see, on examining it, it seems a thoroughly practical thing, and one which in work would give most excellent results. There is only one point as to which I should like to ask Sir Howard Grubb, that is with regard to the arrangement of his pendulum. I rather distrust the giving the impulse at the end of the swing as seems to be the case here. We have a pendulum at Greenwich on our solar clock which is very similar to this. The impulse is given by a sort of electrical gravity escapement of the same nature, and it is rather liable to vary in its rate as the work varies, and of course the work is likely to vary through variations in the contact. As soon as you introduce an electrical contact after a certain time you get sticking, which tends to reduce the arc or to check the pendulum. I do not know whether Sir Howard Grubb has considered whether that can be modified in any form. In any case it is not a matter of vital importance to the clock, because there is no doubt that the pendulum will go accurately enough, even for several hours, to answer the purpose of astronomical photography; still it is as well to have every point as perfect as it can be made.

Sir Howard Grubb. With regard to the remarks of Mr. Common, I quite concur with what Mr. Common says as to the danger and undesirability of moving large heavy masses if it can be avoided. No doubt there is a great deal in what he says in that respect. Of course I am aware if you work the screw of a telescope you get that sort of motion [indicating], and that is precisely what I wanted to try to avoid, and what I claim I do avoid by the arrangement. This is equivalent to an excessively fine screw, so that it means working a screw in the sector about forty times finer than this. It is really but a very slight acceleration or retardation in the clock-motion, and it does not come in very suddenly either, because there is always a slight loss in the teeth of the wheels. I think it will be found in practice, I cannot speak from personal experience, that this slow motion or the automatic one worked by the control will be found to give less of that swinging motion than almost any other way in which it can be done.

With regard to what has been said about the pendulum, I do not think it can be considered that the impetus is given altogether at the end of the stroke. What really happens is this. It takes up the weight at about the centre, lifts it up as far as the end of the stroke, and remains in contact with it till the centre of the stroke has been again passed. That seems to me to be almost equal to giving the impetus in the centre. I do not speak very confidently about that; but I do not think it can be described as giving the impetus at the end of the stroke.

The President. The only point is that it is rather objectionable to be carrying that weight during a large part of the stroke; it is not in the nature of a detached escapement.

Sir Howard Grubb. No.

The President. But still I see now that it is not open to the objection I mentioned.

Sir Howard Grubb. Of course it is understood that this pendulum is not part of the control at all. I merely introduced that part for demonstration purpose, and because I thought, as I said, that astronomers do not like the sidereal clock tampered with. They like to have something independent to drive the equatoreal from. I did not see the use of putting hands on a dial to the clock, so there is simply a pendulum.

The President. If no Fellow wishes to make any other remarks about the driving-clock, I will invite remarks upon the improved form of declination slow motion.

Mr. Common. I should be very loth indeed to criticise unfavourably anything Sir Howard Grubb says, especially after his clock; but there is one error in all these declination movements, and that is that they always bring the effects due to the shape of the rod and the torsion of the tube into play. It would be different if you could have something in the nature of the steadying-rod of the old 3½-in. telescope, which used to be mounted on the alt-azimuth mounting. Andrew Ross used to make such a telescope, with long steadying-rods going from the base of the mounting up to the end of the tube. I need not tell Sir Howard Grubb this, because he knows the errors of this thing as well as I do. There is not the slightest doubt that, as a means of slow motion, as a means of taking up the slackness of the screw, the arrangement he shows is perfect; but as a whole, as a slow motion, it has all the disadvantages of the old form, that is to say, the tremor of the arm, torsion of the axis, and the want of a broad base. In my opinion, the best form of slow motion would be that which gives the nearest approach to the old steadying-rods of the alt-azimuth construction.

Gen. Tennant. Might I just make one remark? it has reference partly to what Mr. Common has spoken of. The clamp, the mode of connecting the movable part with the firm part, is not shown there; and that is a very important thing. There is a good deal of swing in that sometimes, and it is very important to get a really good clamp. I know it can be done; you can get a good clamp, which is much better at one time than at another, but the parts of the clamp that bite on to the limb or on to the movable part of the instrument are very difficult to get hard and fast without any spring. There is a spring as well as a torsion of the axis. The spring is in the bite of the clamp upon the place which it holds on to, wherever that may be. I have noticed very frequently that it is very difficult to get rid of that spring. It does not come exactly within the scope of Sir Howard Grubb's arrangement any more than that which Mr. Common has spoken of. Taking the torsion of the axis and the parts that depend upon it, combined with the spring of the clamp between the two, there is a good deal of difficulty in getting anything like an accurate motion, a satisfactory motion when bringing the object up very slowly. When one has had much to do with geodesic apparatus one feels what is very much needed at times, and knows the difference between the different clamps.

The President. There seems to be a good deal of force in Mr. Common's remarks about the steadying-rod; but there will be difficulty, I think, in applying it to any form but that of the English equatoreal. I should imagine it would involve a great deal of work, and in fact a complete change in the structure of the instrument, if an attempt were made to apply it to the usual form of instrument.

I believe a steadying-rod with a screw giving a slow motion has been applied in small instruments and acts very well indeed, but I should doubt whether it would answer well for large instruments.

Gen. Tennant. Is not the declination of the Northumberland equatoreal very much in that line?

The President. It is so, and there was a somewhat similar plan for the Greenwich equatoreal, but those were really N.P.D. setting-rods, and different lengths had to be used for different declinations. It was found that when these rods were drawn out they were apt to drop on one's toes, or some accident of that kind was likely to happen, which did not conduce either to the improvement of the instrument or of the observer's temper. I must say I have considered this question a good deal with reference to the Lassell equatoreal, and discussed the matter with Mr. Simms, and suggested adopting a scheme of that kind, that is, a sort of steadying-rod, but the practical difficulties seemed to be too great, and on the whole it was considered better to apply a sort of sector arc. I do not see myself that there is any difficulty in attaching the arm firmly if it is attached in the shape of a ring clamp, so that it becomes part of the instrument. I think, with proper regard to mechanical conditions, that sort of arm can be clamped securely, and can be made quite stiff enough. In fact, I should think it might be made stiffer than any steadying-rod. If Sir Howard Grubb would describe the plan he proposed for attaching this clamp, I think it might interest Fellows generally. That is often a weak point in instruments, the clamping-ring being too small in diameter.

Sir Howard Grubb. I think what Gen. Tennant refers to is the old form of clamp, where the clamping is done tangentially. No doubt he is perfectly right that these tangential clamps are certain to move the position in one direction or the other.

(Sir Howard proceeded to describe the working of his clamp.)

There is no doubt that Mr. Common's remarks are perfectly correct in principle with regard to the direction of the clamping. If you could get that indirect form of clamp it would be more advantageous. The tube should be very stiff indeed. If it went to a very great length you would find there was a liability to shaking, and vibration would be communicated to the main tube of the telescope. Altogether I am inclined to think that, though in theory it is no doubt perfectly correct, when you come to work it out it would not be found a practicable form of clamp. The means adapted for getting the best results consist simply in making his clamp as large as possible. For this telescope it, I think, amounts to 2 ft. 6 in. in diameter, so that there is a good broad basis. The arm is made a trussed casting as stiff as possible, and the slow-motion arrangement is attached not only to the tube, but to the flange of the declination axis. It is attached to a bridge on the tube itself as well, so that it has both the tube and the declination axis to drive from. We do the best we can in that way. No doubt Mr. Common went to the bottom of matters, as he generally does.

Mr. Knobel read a paper from *Mr. Eddie* of Grahamstown, on the new Southern Comet. Mr. Eddie described the comet as having the nucleus of a star of the fourth magnitude, with a tail extending eight degrees from the nucleus. A decided curvature could be observed with a concavity towards the south. Mr. Eddie also described the changes which the comet underwent. A paper on the same subject by Mr. Tebbutt was read, and communications by the Astronomer Royal and Captain Clarke of the Steamship "Aldous" were announced.

Thanks to the authors of these papers were returned.

Mr. Knobel read a note on Mars by *Mr. R. A. Proctor*, dealing with the duplications of the canals in Mars.

Mr. Sadler. As a matter of fact the duplication of canals in Mars has been observed at Nice.

Mr. Knobel. With regard to this point I think it is very unfortunate that we have not yet seen any copies carefully reproduced of the drawing which Schiaparelli has made. We possess the chart that he has constructed of the surface of the planet from his drawings, but it would be exceedingly interesting to see carefully reproduced copies of the drawings which he made, and to endeavour to assimilate the appearances which he has seen with drawings made by other observers. And the same with regard to the statement which has just been made by Mr. Sadler, that the duplication of these canals has been seen at Nice. I quite agree with Mr. Proctor that there is a great difficulty in accepting the chart of Mars as depicted by Schiaparelli. The parallelism of the canals is so amazing that one cannot but seek for some explanation as was suggested some time back by Mr. Green, those lines being the boundaries of faint tones and shadows on the planet, or, as Mr. Proctor has suggested, there being some light spaces between certain dark marks which Schiaparelli has interpreted into lines like canals. Certainly I have over and over again recently, as well as at every opposition, failed to see anything approaching parallelism in the configuration of the planet. I have not seen the slightest signs of it, and it does not appear that, with the exception of the observations made at Nice, any one has seen anything like these parallel canals spoken of by Schiaparelli.

Mr. Mathew Williams. There is a question involved in this which has frequently been lost sight of. Such observations as these are not questions of magnifying, and it is rather a curious circumstance that both Schiaparelli's observations and those made at Nice should have revealed details concerning which English astronomers are sceptical. I may mention a case which is illustrative of this. When I was in Malta some years ago, I could see on certain days the summit of Mount Etna at a very long distance. I tried to see it with the telescope and then lost it, though I could still see it with the naked eye. The question was whether it was a cloud or whether it was the white summit of the mountain. By making a mark on the top of the parapet on the line of vision, I became assured that it was not a cloud, from the fact that it did not alter its position in several days. When we are looking at such faint marks as those which have been described as seen upon the planet, the difficulty is about the same as the difficulty was then of seeing such an object as the mountain-peak. It depended on the transparency or haziness of the intervening atmosphere. The glasses of the telescope supplied additional obstruction. It is curious to go back to some of the old observations of Cassini, Bianchini, and other Italian observers on the markings of Venus, which have not been confirmed with larger telescopes, where, again, the observations were made in Italy. I think it is very probable that the clearness of the Italian atmosphere has more to do with it than any magnifying-power of any telescope, and therefore perhaps we should give more credit to these Italian observations than we sometimes do, even although we may be unable to confirm them with larger instruments in our less transparent atmosphere.

A vote of thanks to Mr. Proctor for his communication was agreed to.

The President. There is a very interesting collection of photographs of the late eclipse, taken in Japan, lent by Mr. Levander. There are three photographs of totality, which show a great deal of detail in the corona. I may mention that these three photographs were sent to Greenwich, and that I suggested to the Director of the Observatory that it would be a very good thing if he could send either the original negatives to the Royal Astronomical Society, or positives on glass, which would show more details naturally than these paper prints.

The Fellows will have noticed to-night that we have for the first time the electric light, for which we have to thank Mr. Common, who has superintended the arrangement, and I trust it will be found to be a great improvement on the old method of lighting with gas, and that the ventilation of the room will be very much improved.

As Sir Howard Grubb is here, I would ask him if he would favour the Meeting with some remarks on a question to which he has given some attention lately, namely, the chromatic correction of an object-glass for photographic purposes.

Sir Howard Grubb. I am almost ashamed to occupy the time of the Meeting, but my only excuse is, I do not often come over. I have been working on the correction of photographic object-glasses, and, like all new things, some difficulties present themselves. Anyone who has worked at ordinary optical matters knows how much the trials depend upon the experience of their eye in correction of an ordinary object-glass, but when it comes to correcting a photographic object-glass, the eye ceases to be of use altogether. For a time I was completely at sea, and the question was, how best to get at some measurement of the foci of the different rays. I discussed the matter with Prof. Stokes, and he suggested a number of ingenious arrangements. The first arrangement I tried, which is used more by astronomers than by opticians for testing the chromatic aberration, is that of applying to the telescope a spectroscopic eyepiece, and in that way getting a spectrum of varying breadth, the breadth of the different parts depending upon the amount out of focus of each ray. I think in Prof. Vogel's paper on the Vienna Telescope he gives the results of a number of object-glasses he examined in this way. If I remember rightly, he got a result something like this [drawing on blackboard]. When I came to try this plan, I found considerable difficulty. I found the curves came out very low, the breadth of the spectrum being very small, and I rejected that plan. The plan I have adopted finally, and which works exceedingly well, is this:

I take an instrument which I use for examining the refractive and dispersive power of large discs. It is, in fact, a large spectroscope with a pair of telescopes and carefully divided arcs.

I replaced the observing-telescope by one of very long focus to get a spectrum of large scale, and used a large compound prism which gave me good dispersion.

I used this first for taking the measures of various solar lines. I adopted five or six lines in the spectrum, and I took measures in the daytime of all these different rays. At night I replaced the eyepiece by a short piece of tube with a small convex mirror, and I cut away a large portion of the top of the tube of the observing telescope. At the slit I put a small electric glow-lamp, and I passed a current through this. The top of the tube being cut away, I was able to look at the star almost from the front. The effect was to get a little brilliant star on the centre of about $\frac{1}{50}$th inch diameter, the light of which was due to one particular part of the spectrum only. Then I was able to measure very accurately the focus of the photographic objective for rays of all refrangibilities. That enabled me to construct the curve of the different foci. There is one point about this I have not quite overcome, and it is this, that these ordinates represent the longitudinal foci of the different rays, without allowing for the want of perfect correction of the eye and the eyepiece. A slight correction will have to be made for this.

The President. I can quite bear out what Sir Howard Grubb has said as to the difficulty of determining the chromatic correction of the object-glass by means of the method of applying a prism or small spectroscope near the focus. When applied to a star it is extremely difficult to identify the lines and to tell what part of the spectrum one is examining, and although one can see the general form of the spectrum, it is hard to get precise results. With the Sun we cannot apply that method, because we have not a point to deal with. Another method I have tried has been simply to use a spectroscope to focus the slit on the limb of Sun, moving the whole spectroscope bodily backwards and forwards until the limb of the Sun is in focus at the same time that the lines formed by dust on the slit are in focus at that particular part of the spectrum.

Sir Howard Grubb. Was the slit placed radially?

The President. Yes, radially. I must admit that the method is not susceptible of any very great accuracy. There is a difficulty in defining the exact position for focus on the Sun's limb, and it appears to me likely that this method of Sir Howard Grubb's would be much more accurate. It is a matter of considerable difficulty to get any quantitative measures of the focal lengths for the different rays of the spectrum.

Mr. Common. It appears to me, in making an object-glass for photographic purposes, the first and most important thing is to get your plate and find out to which rays it is most susceptible. The greater range of the new plates is inimical to the photographic object-glass. The plan of using the spectroscope was used by Rutherfurd in 1864. He made his photographic object-glass in that way. He took the spectrum and found out the particular parts of the spectroscope which at focus gave the least width, and selected the particular part he wanted to see; but the plan that Sir Howard Grubb shows is a most important advance on that plan, as any part of the spectrum can be dealt with at will.

The following papers were announced:—

David Gill. "On the Occultations of Döllen's list of Stars, observed at the Royal Observatory, Cape of Good Hope, during the Total Eclipse of the Moon 1888, Jan. 28."

Prof. E. S. Holden. "The Total Solar Eclipse of 1889, Jan. 1, in California; probable meteorological conditions at that time."

J. A. Eddie. "The New Southern Comet; Observations made at Grahamstown, Cape of Good Hope."

John Tebbutt. "On the Difference of Longitude between Mr. Tebbutt's Observatory, Windsor, New South Wales, and the Government Observatories at Sydney and Melbourne."

R. A. Proctor. "Notes on Mars."

Communicated by the Astronomer Royal. "Observations of Comet of 1888 (Sawerthal) made at the Royal Observatory, Greenwich."

John Tebbutt. "Observations of Comet 1888 made at Windsor, New South Wales."

Capt. Jas. Clarke. "Sextant Observations of Comet 1888."

John Edmund Drower, May Trees, Endlesham Road, Balham, S.W.; *Geo. James Newbegin*, Thorpe, St. Andrew, Norwich; and *Capt. George William Read, F.R.G.S.*, 1 Broad Street, Penrhyn, Cornwall, were duly elected Fellows of the Society.

Meeting of the Liverpool Astronomical Society.

April 9, 1888.

J. Gill, F.R.A.S., *Vice-President*, in the Chair.

The seventh meeting of the session was held at the Royal Institution. Fifty-eight members were elected. Mr. F. Wolfe Brenan's motion that the Society should institute periodical examinations in astronomy was carried, the arrangements being left to the Council. A constitution was formally granted to the branch at Douglas, Isle of Man.

A paper on Comet-seeking by Dr. Doberck, Her Majesty's Astronomer at Hong Kong, was read. Dr. Doberck thought that the true cause of the small number of comets discovered in this country was not, as Mr. Denning supposed, due so much to the want of persistent efforts on the part of English observers as to the unsuitable means employed in the search. He thought the best method was to procure a large map, such as Argelander's large atlas, and, comparing the image seen in the comet-seeker with the map, to insert all nebulous objects as they were discovered.

Mr. Denning, in commenting on the paper, thought that Dr. Doberck's view of the case was not upheld by the circumstances attending late discoveries of comets. Dr. Doberck advocated an extremely low power and large field of view; but many telescopic comets were so faint and diminutive that they would elude discovery altogether with such means, while faint, small star-clusters were extremely plentiful, and could not instantly be distinguished from faint nebulæ or comets with very lower power.

Mr. W. Durham, F.R.S.E., read a paper on "Water in the Moon." He observed that the fact that no traces of water have been discovered on the face of the Moon had long been a matter of surprise, as undoubtedly that luminary has been, and is, the seat of volcanic action; and this, from all analogy with such phenomena on the Earth, seems to necessitate the presence of a considerable quantity of water. There are reasons, however, why we cannot obtain evidence of the presence of water, even although it may exist. The Moon, having little or no atmosphere, cannot have dust particles floating about: and it has been shown that these are absolutely necessary for the formation of clouds or rain. Again, the conditions on the surface of the Moon form a most perfect arrangement for evaporating and condensing water with great rapidity. The water, if it exists, would pass bodily as invisible vapour from the bright side and be deposited at once as snow or ice-crystals on the dark side. This action would go on continuously, as the sunlight slowly travels round the lunar surface. In fact, the Sun would, as it were, chase the water round and round the entire surface of our satellite. Mr. Durham considered that perhaps the greater brightness at the edge of the Moon, and the variations in the character of the light which have been observed to occur on her surface, may be due to some such action.

In a note on the "Secondary Light of Venus," Mr. W. T. Lynn, B.A., F.R.A.S., examined various hypotheses which had been put forward to account for this phenomenon. He considered that it was really objective and not subjective, and was due to some illumination in the planet or its atmosphere, and thought it not unlikely that electricity might in some way be concerned in its production; or that, as Arago suggested, the planet's atmosphere may occasionally be pervaded by a luminosity analogous to that which in the Earth constitutes the Aurora Borealis.

A large number of papers were taken as read; and Mr. Gill addressed the students on the distances of the planets.

Meeting of the Royal Meteorological Society.

Wednesday, April 18, 1888.

Dr. W. Marcet, F.R.S., *President*, in the Chair.

Dr. T. E. Hale, B.A., V.C., Mr. R. Lawson, LL.D., F.S.S., and Mr. S. Walker were elected Fellows of the Society.

The following papers were read:—

1. "Jordan's new-pattern Photographic Sunshine-Recorder," by Mr. J. B. Jordan. The improvement in this instrument over the previous pattern of sunshine-recorder consists in using two semi-cylindrical or D-shaped boxes, one to contain the morning and the other the afternoon chart. An aperture for admitting the beam of sunlight is placed in the centre of the rectangular side of each box, so that the length of the beam within the chamber is the radius of the cylindrical surface on which it is projected; its path therefore follows a straight line on the chart at all seasons of the year. The semi-cylinders are placed with their faces at an angle of 60° to each other. They are fixed on a flat triangular plate, which is hinged to a suitable stand having levelling-screws attached, and fitted with a graduated arc as a means of readily adjusting and fixing the cylinders to the proper vertical angle agreeing with the latitude of the station.

2. "On the Meteorology of South-eastern China in 1886," by Dr. W. Doberck. This paper gives the results of observations made at the custom-houses and lighthouses by officers of the Imperial Chinese Maritime Customs. In summer there is very little change of temperature with latitude. The temperature depends upon the distance from the nearest sea-coast, and is greatest at stations farthest inland. The highest mean temperature occurred in July and the lowest in January. The North-east Monsoon blows from September to June, and the South Monsoon

during July and August; the latter does not blow with half the force of the former. Rainfall is greatest in Northern Formosa and least in Northern China. Along the east coasts of Formosa and Luzon the winter is the wet season, while in China July seems to be the wettest month of the year.

3. "Lightning in Snowstorms," by Prof. A. S. Herschel, F.R.S.
4. "Insolation," by Mr. Rupert T. Smith.

The Photographic Chart of the Heavens.

The "Bureau du Comité international permanent pour l'exécution photographique de la Carte du Ciel" has published, amongst other more technical papers relating to this subject, one by Dr. Gill, of a very remarkable character, to which we wish to draw attention. Most of those who attended the Conference understood that the work in contemplation was to make a photographic chart of the heavens, to take pictures of the stars by photography, showing, with the greatest care, the appearance of the heavens as they are at the present time, in order that at a future time these pictures might be used, by comparison with other pictures taken under similar conditions or directly with the sky, to determine the many questions that could be dealt with in this way—to enable, in fact, the astronomer of the future to have the sky of his past and his present to deal with; that this was so will be seen from a consideration of the three following resolutions which were agreed to unanimously by the Conference:—

"1. The progress made in astronomical photography demands that the astronomers of the present day should unite in undertaking a description of the Heavens by photographic means.

"2. This work should be carried out at selected stations, and with instruments which should be identical in their essential parts.

"3. The principal objects are (α) to prepare a general photographic chart of the heavens for the present epoch, and to obtain data which will enable us to determine with the greatest possible accuracy the positions and the brightness of all the stars down to a given magnitude (the magnitude being understood in a photographic sense to be defined); (β) to provide for the best means of utilising both at the present day and in the future the results of the data obtained by photographic means."

These were the fundamental resolutions; others, recommended by the two sections into which the Conference divided, were adopted as explanatory of the first. Amongst these was one in which it was decided to take "a second series of plates down to the 11th magnitude, in order to ensure greater precision in the micrometric measurement of the reference-stars, and render possible the construction of a catalogue." We have stated these fundamental resolutions at length as bearing on the question of a Catalogue of Stars, for the paper by Dr. Gill contains the astounding proposition of cataloguing no less than 2,000,000 stars—that is to say, Dr. Gill gravely and seriously proposes the establishment of a Central Bureau, consisting of Chief, Assistants, Secretaries, and a staff of Measurers and Computers, to take the photographs and *measure* them, and make a catalogue, the work to go on for twenty-five years at a cost of 250,000 francs, or £10,000, per annum, or for fifty years at 150,000 francs.

It is quite true that this is only a proposition that Dr. Gill makes; but if such a proposition is possible in face of these direct resolutions of the Conference, it is quite time that every one interested in the success of the work the Conference met to consider (that is, the Photographic Chart of the Heavens) should bestir himself and see that the proposed work is not endangered by such astounding proposals.

To tack on to a work such as that sanctioned by the Conference,—a work eminently practical, that has the support of all astronomers, and that has already been taken up by many of the Governments who were expected to join,—a gigantic work such as Dr. Gill proposes, a work beside which that proposed by the Conference sinks into insignificance, would neither be fair to the Conference nor just to those Governments who have joined in the undertaking. The feature of the international scheme that makes it possible to obtain the assent of Government is that the work is proved to be practicable by experiment, and that it can be done at a moderate cost in something like five years, while the results are good for as long as the plates will last. To increase this work by extending it to, at the lowest computation of time, twenty-five and possibly fifty years and to add enormously to the cost, would be to jeopardize the whole scheme.

Dr. Gill states that the actual state of astronomical science demands a Catalogue of Stars to the 11th magnitude. He thus raises the question on its merits; and we would here state that it is more than possible that not only is there no need of such a catalogue, but that the use of such catalogues as he proposes has for ever ceased. The minds of some astronomers move in grooves, and it will, no doubt, never be conceded by them that catalogues can be superseded; they will die as they have lived, in the strong belief that the only way to use the stars is to catalogue them.

Till recently the knowledge we had of the stars was only to be gained from a written description of their brightness and position with regard to each other; hence the catalogue was an absolute necessity if we needed to know the number or brightness of certain stars in any part of the sky at any previous time; and we could only find this out if we had a catalogue of that time. Our catalogues of Stars are all we have to show what has been observed up to the present time; but when we have a photographic chart of the heavens, we have for our record not a catalogue, but a representation. That catalogues of Stars such as are used for fundamental places will be always used goes without saying; the photographic plates themselves, and the 4 or 5 stars on each required as the fiducial points and for identification, will of course be catalogued; but, beyond this, to catalogue the stars on each plate, to measure them for the purpose only of getting their places written down, would be the most utter waste of time, labour, and money that it could enter the mind of man to conceive.

The proposition brought forward by Dr. Gill should be settled decisively so far as the proposition concerns the work proposed by the Conference. There can be no question that such a thing was never intended; had such a thing been thought of, we should have had a "Conference for discussing the best way of making a Catalogue of Stars by photography."

As this was not done, it can be done now; and if there is the great need of a Catalogue of Stars to the 11th magnitude felt by so many astronomers, as stated by Dr. Gill, it is a thing of so much greater importance as far as cost and time are concerned, that it should be considered, and dealt with entirely apart from the other work. A new Congress might discuss it; the one which met in 1887 is not in any way committed to such a scheme.

We hope to refer to this subject again next month.

EDITORS.

Selenographical Notes.

UNNAMED FORMATIONS WEST OF BULLIALDUS.—In the Mare Nubium, about midway between Bullialdus and Guerike, and extending from S. lat. 12° to 19°, there is a group of formations which is scarcely noticed by selenographers and is indifferently represented in lunar maps, though at sunrise it is very conspicuous, and is associated with one of the finest examples of rill-like valleys to be found on the Moon's visible surface. On March 21, when the E. long of the morning terminator was about 18° 30′, this region was well seen with a power of 340 on my 8½-in. Calver reflector, the bright plateau east of Guerike, with the two quasi-ring-plains between it and Bullialdus D, forming an especially striking and beautiful group, worthy, by reason of its size and definite characteristics, of a distinctive name, which at present is wanting. That which rendered, however, these formations still more noticeable was the rill-like valley which traverses them from north to south, grazing the west side of Bullialdus *e* in its course, and, becoming more delicate and like a true cleft, ultimately trending, I believe, towards two craterlets some little distance north of Nicollet, in south lat. 20°. At its northern end it abuts on the north-east corner of the wall of the more northerly of the two enclosures, and runs up to the western side of the conspicuous crater Bullialdus *e* in the form of a straight, deep, coarse furrow, bordered on its eastern side by a low bank. Beyond this, in its progress through the second enclosure, it bends very slightly towards the west, and exhibits three or four very palpable crater-like expansions. Here the bank on its eastern side becomes wider, and casts a more pronounced shadow up to a point about five diameters west of Bullialdus D, where the valley contracts suddenly and, assuming the character of a true cleft, traverses a spur projecting from the west side of a bright, but undesignated, crater situated on a ridge some distance west of Bullialdus D, and then, curving towards the south-west, becomes more and more delicate till it is only visible in moments of the best definition. I can find no trace even of the coarsest section of this very remarkable valley either in Mädler's or Neison's map. Schmidt shows the northern portion of it up to Bullialdus *e*, but incorrectly. Beyond this, his chart, through the faults of reproduction, is very unsatisfactory, so that it is difficult to distinguish the true outline of the second enclosure amid the confusion of detail shown; but nothing pointing to the existence of a narrow valley or cleft is indicated.

The fine lofty triangular plateau east of Guerike B was a beautiful object at sunrise on March 21, the brilliant little crater *k* on its western flank and the lofty peak at its northern extremity being the most conspicuous features thereon. A little to the south-east of the latter there is a large irregular formation, resembling an incomplete ring. It has a bright crater on its northern side. On the east of this ring runs a short cleft (S 302; ϕ, Neison, Map xiii.) towards the south-west, crossing the plateau nearly from side to side. What appeared to be a second cleft was noted branching at right angles from it and trending in a south-easterly direction.

There are many craterlets in this region of considerable size which are not shown in the maps, and some of the largest which are set down have no distinguishing letters attached. Considering its somewhat central position on the disk, this neighbourhood seems to have been surveyed and mapped in a very slovenly fashion, though it includes many interesting details in addition to the curious rill-valley which traverses it.

Kempston, Beds, 1888, April 19. THOMAS GWYN ELGER.

CORRESPONDENCE.

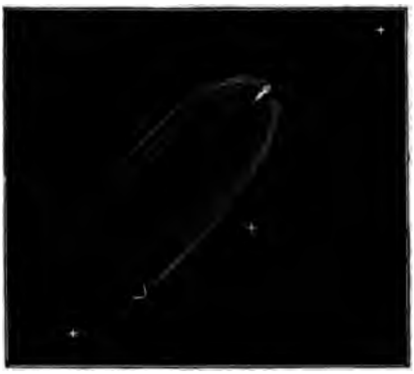
To the Editors of 'The Observatory.'

Comet Sawerthal.

GENTLEMEN,—

This comet, discovered by Sawerthal on Feb. 18, was observed by me this morning with the 9-inch silver-on-glass reflector, and found to be a conspicuous and interesting object. The sky was exceedingly clear, howbeit the first clear morning sky we have had since the comet came into visibility in northern latitudes.

The comet was just visible to the naked eye when well located. In the telescope it had a very bright nucleus much elongated, and a wide tail about one degree in length.



The above sketch gives the appearance of the comet as seen with a power of 60 diameters. Its approximate place at this observation was R.A. $21^h\ 40^m$; decl. south 5° 15′.

WILLIAM R. BROOKS.

Red House Observatory,
Phelps, N.Y., U.S.A.,
1888, March 25.

The Visibility of the Unilluminated Part of Venus.

GENTLEMEN,—

With reference to Mr. Elger's letter in the 'Observatory,' No. 135, I may perhaps be forgiven for copying verbatim a portion of the record of an observation made by myself on 1876 May 18. After detailing in my Observatory-book certain experiments undertaken to test the validity of a theory then recently broached as to specular reflection from Venus, I find myself going on to say—"Changing the power now for one of 154, I see that the blue light surrounding Venus (and having its origin in the slight over-correction of the object-glass) perfectly simulates a phosphorescent disk, or dark limb rendered visible by the *lumière cendrée*. This, however, is an obvious optical illusion, and is totally different from the dark limb seen in bright sunshine." I need scarcely add that the concluding clause of the quoted sentence had reference to observations of the unilluminated limb of Venus when in, or close to, inferior conjunction, which I had observed on previous occasions, and have seen on more than one since. In fact, as it happens, I have only to turn over one leaf of the book from which I have just quoted, to find an entry germane to the matter. It occurs under the date of 1876 July 14, when Venus was actually in inferior conjunction; and it would appear that I had the Rev. Professor Wiltshire as a visitor to my observatory, for I wrote thus. After speaking of showing him the planet, I say that I did so "with a request that he would look and see if there was any difference of shade in any part of the planet; and after looking at it for a minute or two, he said 'the part within the cusps is decidedly *darker* than the firmament.' I quote his words verbatim as affording valuable independent testimony to the objective nature of the appearance, as he knows nothing of observing and had no preconceived theory to support." I may add (what I have stated orally on more than one occasion at the Meetings of the Royal Astronomical Society) that I believe that, under favourable atmospheric conditions, the phenomenon of which I am now speaking may be seen whenever the planet is in inferior conjunction; my own theory being that her unilluminated limb is rendered visible by projection upon some extension of the solar corona. To see it properly all extraneous light should be cut off; and I have found a cardboard disk, fitted on to the diaphragm between the lenses in an ordinary Huyghenian eyepiece and perforated with a minute hole made with a red-hot needle, a most effective contrivance for this purpose.

Faithfully yours,
William Noble.

Marshalls Manor House, Maresfield,
Uckfield, 1888, April 9.

Intersecting Rainbows.

Gentlemen,—

In No. 1 of the Publications of Morrison Observatory a Note was inserted on "intersecting rainbows," observed here in July 1884. As this phenomenon (on the assumed hypothesis) must always occur under a certain rare combination of conditions, I expressed surprise in that note that, in regions abounding in small lakes, no observations of it (so far as I knew) had been made. I am glad that the publication has served to bring to light at least *one such observation*, made 83 years earlier; and though the subject-matter belongs rather to Physics than to Astronomy, yet as astronomers, *in every instance*, have been connected with the observations, I hope that, in the interest of science, I may ask for the publication of this note and of the accompanying letter of Herr H. Geelmuyden, Assistant Astronomer at the Observatory of Christiania. It may be that there are *other observations*, and the publication may serve to bring them also to light, and even to compel writers on elementary physics to do *full justice*, at least, to the Rainbow. The Norwegian astronomer, Christopher Hansteen, will be remembered not only as the director and founder of the observatory at Christiania, but also as one of the most vigorous writers on magnetism in the earlier years of this century.

[Copy of the Letter.]

"Christiania Observatory, 1887, Dec. 20.

"Sir,—In the Publications of the Morrison Observatory, No. 1, I see a note on two intersecting rainbows, observed 1884 July 24 at your observatory. Perhaps it may interest you to know of a similar observation made many years ago by the late Professor Hansteen, and described by him in 'Magazin for Naturvidenskaberne,' Vol. i., Christiania, 1823. The description begins as follows:—

"'1821, Aug. 10, near the Church of Slidre, in the valley of Valders (Norway), at 7 P.M., shortly before sunset, I saw two rainbows intersecting each other near the horizon and near the east point, at an angle of 20 or 30 degrees, and then withdrawing further from each other in greater altitude. None of the bows were complete, about the half on the right hand (towards south) wanting. It was most remarkable that the colours in both followed in the same order, as in the primary rainbow, and that both had equal intensity; so that neither I nor my fellow travellers could decide which was the genuine one (*i. e.* that whose centre was opposite to the Sun) and which the mock one. The Sun was already hidden from us by some high mountains towards the north-west.'

"The explanation given by Hansteen is exactly similar to that suggested in your case. West and north-west of Slidre is the long and narrow Strudefjord (an extension of the river of the valley) enclosed between mountains, which generally protect it against the wind, and thus give it a smooth reflecting surface. Consequently the lake may have produced the reflection, giving the uppermost of the two rainbows. By calculation Hansteen then proves the correctness of this hypothesis. "H. GEELMUYDEN,
Assistant Astronomer."

In America we often recur to the lakes of Scotland and Switzerland for our ideals of placid sheets of water. They are small; and unless the mountains approach too close to their shores (so as to intercept either the direct or reflected rays of the Sun at low altitudes), I should expect to find in their vicinity some trace of similar observations in the past. Will not the scientific journals take sufficient interest in the matter to elicit all the evidence available?

C. W. PRITCHETT.

Morrison Observatory, 1888, Mar. 8.

The 'Nautical Almanac.'

GENTLEMEN,—

On the subject of needed improvements in the 'Nautical Almanac,' it appears to me that the explanations for the tables on the Moon's libration are not sufficiently full, especially as regards the way the signs + and − are to be interpreted with reference to the longitude. It also seems rather strange that the explanations given are only to be found in the issues 1867–69, so that any one not possessing those volumes will not be able to use the tables. No doubt those who are in the habit of using these tables will understand them; but might we not reasonably expect more satisfactory explanations, more especially as most astronomical books do not refer to the subject at all?

Yours faithfully,

Sunderland, 1888, Mar. 26. T. W. Backhouse.

The Total Eclipse of B.C. 400.

Gentlemen,

In the 'Monthly Notices' for January, 1857, Prof. Hansen discussed the solar eclipse of B.C. 400, June 21 (he calls the year 399, evidently meaning −399=B.C. 400 in ordinary chronology), and finding that, according to his calculation, it was total at Rome about half-past 7 in the evening, concluded that it was the eclipse referred to by the poet Ennius in the fragment quoted by Cicero in his 'De Republicâ,' i. 16. Niebuhr had before taken the same view in his Roman History; he took his astronomy from Heis, who thought that the eclipse was not visible at Rome in its totality, but was so in the south of Spain, whence intelligence might have reached Ennius. It was thought by Niebuhr that an explanation would thus be furnished of the peculiarity of the expression used by Ennius, "soli obstitit luna et nox," as implying that night closely followed the eclipse, *i. e.*, that it occurred late in the evening. Sir G. Cornewall Lewis considered this explanation "fanciful and far-fetched," and though Hansen accepted it, I must confess that it seems to me doubtful; the "nox" may merely refer to the darkness caused by the eclipse.

There is another point. No historic event is connected with this eclipse. Ennius says that it occurred in the Nones of June; and though both Niebuhr and Hansen do refer to the confusion of the Roman calendar in the time of Ennius, they scarcely seem to make sufficient account of it. Two eclipses are mentioned by Livy, one of which occurred during the life of that historian, the other the year after his death. The first of these took place whilst the Apollinarian games were being celebrated in the year A.U.C. 564, corresponding to B.C. 190. Livy gives its date as the fifth day before the Ides of Quintilis, which should be June 10; yet we know the eclipse (which was solar) took place on the 14th of March. The second was a total eclipse of the Moon, which occurred on the eve of the battle of Pydna. The date of that event was B.C. 168, and the battle is said to have been fought on the day before the Nones of September, which would make the eclipse on Sept. 3; yet we know that it took place on the 21st of June. It would seem, therefore, that the calendar at that time was about 2½ months behind the truth. Of course it does not follow that it was as much so when the eclipse mentioned by Ennius took place; but, on the other hand, the account is so isolated (owing to the brevity of the fragment preserved by Cicero) that one cannot help suspecting that the figures may be wrong, which state that it was 350 (quinquagesimo ccc.) after the building of the city. The year B.C.

400 would correspond to A.U.C. 354 according to the accepted reckoning, which, however, we cannot be sure that Ennius followed.

I cannot leave the subject without referring to the conjecture of Mr. Johnson, in his excellent little book on 'Eclipses Past and Future,' that the battle of Pydna and the eclipse of the Moon which preceded it took place not in B.C. 168 but 167. His principal reason for this is that Plutarch says that the eclipse came on when the Moon was high in the heavens, which applies better to that of June 10, B.C. 167, than to that of June 21, B.C. 168. I think, however, that the original Greek word *μετέωρος* does not at all imply that the Moon was high, but only that it was well up in the sky, when the complete obscuration came on; and, on the other hand, as Plutarch says that the troops were just preparing for sleep after their principal meal, that scarcely agrees with the eclipse of B.C. 167, which did not commence in Greece until midnight. I see no reason, therefore, for altering in this way the accepted date of the battle of Pydna. But this little affects my argument respecting the confusion in the Roman calendar at the time, which makes it impossible, without other guidance, to fix the date of the eclipse referred to by Ennius.

Yours faithfully,

Blackheath, 1888, Mar. 23. W. T. LYNN.

OBSERVATORIES.

WOLSINGHAM OBSERVATORY.—Circular No. 19. The star DM. +40°, No. 2694, R.A. $13^h\ 24^m\ 43^s.2$, Decl. +40° 15′·9 (1855), Mag. 9·2, was observed here on the nights of April 6 and 8, as 7·3 and 7·7. It seems, therefore, variable. The spectroscope shows a fine III-type spectrum. T. E. ESPIN.

1888, April 9.

Mr. Sadler writes:—"The place of D.M.+40° 2694 should be $13^h\ 42^m\ 43^s.3$ +40° 15′·9 (1855), and not as printed above. The star is not in Lalande or Weisse's Bessel, and I have failed to find it in any modern catalogue. The place in D.M. is taken from B.B. vi., where it occurs as a 9·2 magnitude star; the place for 1855·0 being $13^h\ 42^m\ 43^s.32$ +40° 15′ 55″·9, from a single observation with the Bonn circle on April 23rd, 1858."

PUBLICATIONS.

OLD AND NEW ASTRONOMY.—We have received the first instalment of a new work with this title by Mr. R. A. Proctor. The 64 pages quarto which form Part I. suffice for the Introduction and first chapter. We do not gather from the titlepage how many parts are to follow. The scope of the work may be best described in Mr. Proctor's own words :—

"It is as a subject for study and contemplation as a means for training and exercising, but likewise for ennobling and purifying the mind, that astronomy should be studied by all whose duty it is to deal with the heavenly orbs in another way. The astronomical surveyor must work unmoved by such thoughts, or he will scarcely work in an effective manner. The meaning of the stupendous celestial mechanism, the beauty and harmony of the celestial architecture, it is not for the Flamsteeds, the Maskelynes, and the Airys—useful, nay, essential though their work may be—but for the Newtons and Herschels of astronomy to investigate. It is the celestial scene as viewed and studied by philosophers such as these, not merely as surveyed in Government observatories, thatI propose to contemplate in the present volume: for astronomy regarded as a means of philosophic training, owes almost all its value to men of the former type, scarcely any (though commerce owes much) to those of the latter."

The Introduction is chiefly devoted to the exposition of this principle of contemplation, which is Mr. Proctor's particular province. He makes some good remarks on the possibilities of developing our conceptions of great distances and long periods of time; but he is not always clear on the point which is to be "clearly apprehended," as for instance in saying that "the whole family of the minor planets could not span the distance between the orbits of Jupiter and Saturn."

The first chapter, on Methods of Observing the Heavenly Bodies, deals with astronomical instruments, from the Tower of Babel and the Great Pyramid to the Transit-circle, and contains nothing essentially new. It is impossible to criticize a book from the first number, but Mr. Proctor's admirers will no doubt look forward with interest to the appearance of subsequent parts.

IN PURSUIT OF A SHADOW *.—A pleasant little book, detailing the travels of two ladies who went to Russia last year in order to observe the total eclipse of Aug. 19. Although they unfortunately met with an almost total disappointment, the tone of the book throughout is bright and cheery; the first thought and question after the eclipse was over was, "When and where will the next be;" and the little volume ends with referring to the "hope that *would* not be wholly crushed of better success yet to come." Nevertheless all lovers of our science will give the authoress their heartiest sympathies, for the disappointment, though so bravely borne, must have been keenly felt. And the book is so pleasantly written, the descriptions so fresh and natural, and with such an absence of affectation or strain after effect, that one cannot but regret that the writer had not the opportunity of recording some

* 'In Pursuit of a Shadow.' By a Lady Astronomer. (London: Trübner & Co., Ludgate Hill.)

in asking those present to visit the Lodge, where he had collected various pictures of Newton, the comparison of which was extremely interesting.

TOTAL LUNAR ECLIPSE OF JANUARY 28.—Dr. E. Lindemann has forwarded the following additional reports of the number of occultations observed during the above eclipse:—

	No. of Obs.
Bonn	7
Bruxelles	14
Liége	5
Albany (U. S.)	7
Radcliffe, Oxford	9
Milan	23
Greenwich	14

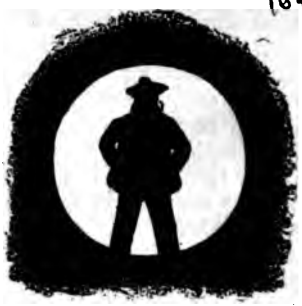
79

It was cloudy at Warsaw.

	No. of Obs.
Madras	10
Cape	21
Durban (Natal)	17
Palermo	8
Christiania	28
Sunderland	1

85
79
164

APPENDED is a sketch of a very remarkable figure given by the 5-foot reflector at Ealing. It was obtained by Foucalt's method of testing, viz. rays of light thrown from a lamp placed at twice the focal distance, and projected back unto a screen. Various theories have been put forward to account for this singular phenomenon, for the most part highly unsatisfactory. V. C.



PROF. HOLDEN AND MR. PROCTOR.—Mr. Maunder has received a letter from Mr. Proctor, apparently intended for the Editors of the 'Observatory,' with reference to a paragraph quoted in the 'Sidereal Messenger,' from our issue of March last, p. 168. We regret that the intemperate tone of this letter does not allow us to print it as it stands, more especially as this course may lay us open to a charge of injustice. But on consideration we cannot recognize that the standing right of reply is more imperative than considerations of what may be properly and decently inserted in our magazine.

For the clearer exposition of the matter we must recall the following facts:—

1. Mr. Proctor addressed a letter to the 'Sidereal Messenger' which was printed in 1887 September, and with one sentence of which, and that only, we have anything to do. Since Mr. Proctor

lays some stress on the context, however, we may say that the letter was generally directed, in no measured terms, against Prof. Holden. The sentence in question was as follows:—

"Prof. Holden owes it entirely to me that his name when suggested for Foreign Associateship of the (Royal Astronomical) Society was not rejected with contempt."

To every one whom we (either at the time or since Mr. Proctor's late explanation) interrogated on the subject, this sentence only admitted of one interpretation—viz. that it was owing to Mr. Proctor's exertions in his favour that Prof. Holden was elected; and as this statement nearly concerned the honour of the Astronomical Society, Mr. Knobel officially, and others privately, wrote expressing their surprise at this statement in face of the fact that Mr. Proctor was not only not on the Council of the elective body at the time of election, but not in England. We expressed our opinion of this course, and our expectations from Mr. Proctor, on p. 168 of the March number, concluding with the remark: "We trust that Mr. Proctor may be induced to offer something in the way of explanation and apology, though it is difficult to understand how anything less than an unreserved withdrawal of his statements will meet the case."

Mr. Proctor's explanation is as follows ('Sidereal Messenger' for April, p. 174):—

"Mr. Knobel's letter is correct in every detail. But I am at a loss to understand why he or you (Editor of the 'Messenger') should regard it as involving a contradiction of anything in my letter. Especially he is right in saying that I did nothing positively to advance Prof. Holden's election. My letter should show that I *could* not have done anything of the kind. I repeat my statement, however, that 'but for me' ('my silence,' I should have said, if I had supposed it possible I could be in any degree misunderstood) Prof. Holden's name would have been rejected."

Mr. Proctor has now clearly shown us that the words were originally intended to bear this meaning; for he has sent us cuttings from the 'Examiner' of a letter written by him on 1887 April 15, of the same general tenour and tone as that which appeared in the 'Sidereal Messenger' for September last; and in which the sentence which has caused those concerned for the Royal Astronomical Society all this trouble was somewhat expanded as follows, if Prof. Holden will forgive us for quoting it again:—

"The Society did elect him as one of its Foreign Associates, but he has to thank my generosity, or rather my contempt, that his name was not disastrously rejected. He has only to look down the present Council-list to perceive that I have, and always have had, friends enough in the Society to utterly destroy any man's chance of such election who had been guilty of the wrong-doing he committed had I cared to put out my hand. I held my hand, and because of his official position he passed through."

But our difficulty is now a dilemma; either Mr. Proctor's damning secret existed in his imagination only, or he withheld from his "friends" a matter of serious importance to them.

COMET *a*, 1888 (SAWERTHAL).—The comet has been observed at Greenwich on four nights only, viz. April 6, 18, 26, and 29, the weather having been almost continuously bad in the early morning. On April 6 only was any trace of a tail visible, and the tail was then seen fairly well. It was very different in form from that drawn by Mr. Brooks on p. 228 of this number, being straight, narrow near the head, and broadening till lost in the sky light at about half a degree from the head.

The spectrum was examined by Prof. Riccò on March 14, by Prof. Tacchini on March 27, and by Mr. Maunder on April 19. All three observers agree in stating that the comet gave a faint but fairly broad continuous spectrum, crossed by three faint bands corresponding to the well-known carbon bands characteristic of cometary spectra in general.

Ephemeris for Berlin Midnight*.

1888.	R.A. h m s	Decl. ° ′	Log *r*.	Log Δ.
May 2..	23 27 49	+27 39·8	0·0579	0·1975
6..	37 5	29 43·3	0·0790	0·2111
10..	45 56	31 37·4	0·0993	0·2237
14..	23 54 23	33 23·4	0·1189	0·2355
18..	0 2 24	35 2·1	0·1376	0·2465
22..	10 0	36 34·4	0·1556	0·2567
26..	17 11	38 1·0	0·1728	0·2661
30..	0 24 0	+39 22·6	0·1894	0·2747

AUTHORS' CORRECTIONS.—DR. TERBY asks us to say, with reference to his drawings of Saturn (No. 135, p. 195): "La zone brillante située entre la projection de l'anneau obscur et la bande sombre principale du globe est trop fortement ombrée, et, au contraire, il faudrait ombrer la région polaire nord dans le premier dessin, la partie du globe qui dépasse l'anneau au nord étant trop claire."

In MR. LYNN's letter, No. 135, p. 197, line 2 from bottom, for 26th read 25th.

MR. BRYANT says:—"In the last number of the 'Observatory,' General Tennant seems to have misunderstood my previous note. He says he would not call a sextant having accidental errors of 14″ a well-divided instrument. I did not say so. What I say is that with a well-divided instrument reading to 10″, the accidental error of a single reading is 14″."

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* Astron. Nach. No. 2835.

Focal Lengths for different Rays of the Spectrum.—It is often a matter of importance to determine the distance separating the foci of an object-glass for different colours, as, for instance, in the adaptation to photography of ordinary objectives by the addition of another lens or lenses. The straightforward method of focussing a spectroscope on successive lines of the spectrum is open to the objection that the focal adjustment of the eye changes during the measures, though in some recent experiments made at Greenwich no serious inconvenience was felt from this cause. Herr Max Wolf has, however, devised an ingenious method of overcoming this difficulty. A bright point (formed either by a spark or sunlight on a drop of mercury) is placed in the focus of the object-glass and a plane mirror behind the object-glass. The light from the bright point, after passing through the object-glass, is reflected through it again by the mirror, and comes to a focus in the neighbourhood of the bright point, the point and its image being accurately in the focal plane when the plane containing them is perpendicular to the axis of the telescope. If the two points are now viewed with a spectroscope, the focus for (say) the F line is found by moving the point until the F lines in the two spectra (of the point and its image) are *equally* well defined, which eliminates the eye as an absolute judge of definition.

We have received the following letter from the Secretaries of the Royal Meteorological Society:—

Sirs,—The Council have received from Dr. L. Cruls, Director of the Imperial Observatory of Rio Janeiro, a circular letter addressed to all meteorological observers.

The circular states that the Imperial Observatory contemplates the preparation of a Universal Dictionary of Climatology, containing data relating to the principal elements of the climate of every country of the world.

Forms for filling up have been forwarded to the Society, and can be obtained from Mr. W. Marriott on application.

Dr. Cruls states that the Imperial Observatory will present two copies of the work to any gentlemen who will furnish information—a liberality which is very unusual.

We are, Sirs,
Yours faithfully,
G. J. Symons, F.R.S., } *Hon.*
John W. Tripe, M.D., } *Secs.*

30 Great George Street,
Westminster, S.W., April 1888.

Minor Planets.—No. 269 has been named *Justitia*. Herr Palisa discovered No. 274 on Apr. 3, No. 275 on Apr. 15, and No. 276 on Apr. 17.

Sir Howard Grubb delivered a most interesting lecture on Telescopes for Stellar Photography to the Society of Arts on Wednesday, April 18. He referred to the ever-increasing importance of instrumental equipment, especially in the case of photography, where so much depends on the excellence of the driving-clock. He proceeded to describe several methods for regulating clocks by "electrical control," pointing out that some such form of regulator must be used if the *error* of the driving-clock is to be corrected, and not merely its *rate*, as is the case with friction- or resistance-governors.

By combining the good points of previous forms of control, Sir Howard Grubb claims to have devised a really satisfactory apparatus, which will keep the errors of the driving-clock within $0^s.05$. We congratulate him on his achievement, and hope that he will soon have an opportunity of demonstrating the success of his control in observatory work. The lecturer further referred to the optical questions to be considered in selecting a telescope for the work: dwelling on the importance of a large field (a problem hitherto not solved because only recently presented for solution), and an ingenious method devised by himself for determining the focal lengths of different rays with a view to obtaining the maximum photographic effect. He also referred to Prof. Stokes' suggestion for an object-glass which could be used either visually or photographically by reversing the crown lens, "which was described last spring at the Royal Astronomical Society, but has since been re-invented in America."

The Astronomer Royal, who was in the Chair, invited a discussion on the lecture, in which Prof. Stokes, Messrs. Common, Crossley, Criswick, Stoney, Sadler, Taylor, and Stromeyer, and the Chairman took part. A full report of the lecture is given in the 'Journal of the Society of Arts' (price 6*d.*), No. 1848, published by George Bell and Sons, York Street, Covent Garden.

We are glad to find that the arrangements for lighting the rooms of the Royal Astronomical Society by electricity have been successfully carried out. The light was used at the last meeting of the Society to the general satisfaction.

The extension of the Computing-rooms at the Royal Observatory, Greenwich, is now completed; and the rooms were occupied for the first time on Monday, April 23rd.

Mr. J. Gill is Vice-President of the L. A. S., not President as stated on p. 181 of No. 135.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 137. JUNE. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, May 11, 1888.

W. H. M. Christie, M.A., F.R.S. (Astronomer Royal),
President, in the Chair.

Secretaries: E. B. Knobel and Col. Tupman.

The Minutes of the previous Meeting were read and confirmed.

Col. Tupman said that 55 presents had been received since the last Meeting, amongst them being the first volume of the 'Publications of The Lick Observatory.' It contained a reprint of Mr. Burnham's double-star measures with the 6-inch telescope, already published in the 'Monthly Notices.' Miss Wylie presented the Society with a copy of the 'Eclipses recorded in Chinese Works.' It had been printed but never published.

A vote of thanks to the donors was passed.

Mr. Knobel read a paper entitled "Remarks on Sir George Airy's Numerical Lunar Theory."

The President said it was a very technical subject, and perhaps it would be better to study it in print than to attempt to discuss it merely after hearing it read.

Mr. Knobel laid before the Meeting a paper on "The condition that in a double-image micrometer the value of the revolution of the micrometer-screw be independent of the accommodation of the eye," by Professor J. A. C. Oudemans. Mr. Knobel observed that this was a mathematical investigation of the subject which would not be suitable to read; but the conclusion of the investigation was to the effect that it might be safely concluded that in a double-image micrometer, constructed according to the proportions originally pointed out by Valz, the value of one revolution of the micrometer-screw was practically independent of the accommodation of the observer's eye.

The President. It strikes me that this is a very valuable paper, as illustrating a point of much interest in the application of an improved form of micrometer eyepiece. The theory of the double-image micrometer is beset with various difficulties. The question has been raised as to whether the scale value is the same throughout the whole extent of the field, and there is this further question whether the value of the scale is altered by adjusting for vision for different eyes, short-sighted or long-sighted. It seems a very satisfactory result that Professor Oudemans has arrived at. I may perhaps express my opinion that, as far as I am aware, this subject of the adjustment of eyepieces to eyes of different vision has not been sufficiently attended to in the discussion of optics.

Mr. Sadler. I think you will find that the matter was investigated by Prof. Kaiser in the Leiden Observatory about 14 or 15 years ago, in regard to the adjustment of a double-image micrometer.

The President. Kaiser published a very interesting memoir on the scale-value of the double-image micrometer in the Leiden Observations, but I am not aware of his having discussed the question of its applicability to long- and short-sighted persons.

Mr. Crossley then laid before the Meeting a paper describing his new Observatory with the 3-foot Newtonian silvered glass reflector mounted in it, and observed that it was perhaps more suited for printing in the 'Notices' than for reading, especially as he was not provided with drawings suitable for a large room. On the removal of Mr. Common's 3-foot reflector from Ealing, the ingenious wooden house or observatory designed by Mr. Common was also re-erected at Halifax; but as the situation at Halifax was more exposed than at Ealing, it was found desirable to put up a dome, so as to more effectually protect both the instrument and the observer. It would be seen at once, in working at any altitude with the old observatory, that nearly the whole of the telescope was entirely exposed to the wind, and therefore liable to vibrations, and the observer had to take care of himself. It was something like a man without a coat on a winter's night. The construction was admirable, both for economy of material and for strength, and also for working in every respect, except that of protection. The present dome was completed in 1887, and was constructed in a similar manner to that at the Greenwich Observatory, except in so far as the covering and the shutters and some minor details were concerned. The building itself was circular, 38 ft. 9 in. in diameter outside, and 36 ft. 3 in. inside; the walls being 15 in. thick. Five windows and a north door gave access to ventilation. A small room in the north-west corner with flat roof contained a hydraulic winch for moving the dome. A cast-iron rail ran round the whole of the wall in 24 segments bolted together and to the wall. The foundation or ring of the dome was made of ¼-in. boiler-plate with 4-in. angle-iron, and carried 14 wheels, upon which the observatory moved, the wheels turning in solid cast-iron boxes. The dome itself was constructed of 23 ribs, the 24th being left out for the opening. These were supported by two strong parallel girders 6 ft. apart, made of boiler-plate and 3-in. angle-iron. The ribs were also bound together with three circles of 2-in. angle-iron, and the sheathing of the dome was of galvanized iron, $\frac{1}{12}$ in. in thickness, rivetted to every rib, so that the full strength of the material was given to the dome. The shutters differed from those at Greenwich, where the shutter was all in one, with ribs going right across the dome and meeting at the opposite side at a centre point, so that the shutter moved with a radius equal to the diameter of the dome. He thought it was rather a risk to attempt that method in this case, and that it was safer to keep to the parallel shutters, and the two parallel shutters 3 ft. in width were made to run on rails top and bottom. Mr. Crossley then proceeded to explain the mechanical arrangement by which both shutters could be opened and closed simultaneously. The movement of the dome was so arranged that it could be turned round in five minutes, or the operation could be extended over 24 hours. The movement of the dome could be controlled by the observer with perfect ease without leaving the telescope. The weight of the dome was about 14 tons, and the observing gallery and sliding platform attached to the dome would weigh 1½ or 2 tons. Therefore the whole weight would be from 15 to 16 tons; but the water engine was quite capable of turning it round, and as the observer stood upon the observing platform and worked the dome round, he experienced something like the motion of a vessel floating on the water.

The most convenient mode of preserving the instrument was no doubt by means of hot-water pipes; just as much heat could be used as was necessary to keep the moisture from depositing on the instrument. When it was wanted to use the instrument, the hot water could be turned off and cold water run in in a moment, so that was no obstacle to observing. As a means of preserving an instrument safe and sound, he could strongly recommend the use of hot water. As to ventilation, the openings for ventilation were equal to about one-ninth of the area of the dome, and in sluggish weather it would be easy, if necessary, to provide ventilation by artificial means, as, for instance, by setting the water engine to drive a little fan either sucking the air out or driving it in, and so changing the whole air of the dome in a very short space of time. It was not his intention at the present time to say anything about the telescope itself. He hoped at some future time to give some information to the Society about the telescope and its work. The construction of the dome had been very ably carried out by Messrs. Clapham Bros., an engineering firm at Keighley, this being the first astronomical dome that they had constructed. Mr. Crossley also described an arrangement of forks by which the ropes from the winch were kept clear of the dome all round on the outside. If in contact with the dome, he explained that the wire ropes would catch the rain-water and soon rust and become worthless, but if well tallowed and kept clear from the dome, they would last a long time. Another point was the snow-guard running all round, which was to prevent the snow from lodging in the rain-trough and blocking up the dome. One defect in the rails on which the shutters moved was that they were troughed instead of being arched, and he found that during the recent snowstorms the snow got into them and froze hard, involving a little labour in order to get the shutters clear. But it was easy to arrange a cover so that the snow should not get into them. He had no reason whatever to be dissatisfied with the working of the dome. Being of iron it rapidly cooled, and, so far as he had been able to judge, there was no serious difficulty in reference to temperature. With regard to the telescope itself, he had given a good deal of attention to minor improvements with reference to its adjustment, which he hoped to refer to in a future paper.

The clear aperture of the dome was 5 ft. 6 in. wide from the horizon to 3 ft. beyond the zenith.

The President. It is somewhat striking that owing to the form of mounting, this telescope of comparatively short focus requires a 40-foot dome, whereas we have been able at Greenwich to house the Lassell 20-foot telescope in a 30-foot dome.

Mr. Crossley. Of course that is the drawback to that form of mounting, but at the same time there are advantages in it, so it shows that when you get an advantage in one direction, you may lose it in another.

Mr. Turner. It would appear that great improvements have been made by Messrs. Cooke and Sons and others in building domes. When in Russia last year we found that the dome for the great refractor at Pulkowa was hardly so satisfactory as could be wished. Firstly, it is very heavy, and takes an engine to pull it round; and, secondly, they do not seem to have solved the question of making it water-tight, for they had little pans in various places to catch the drippings. These new lighter domes, such as the Lassell dome at Greenwich, have proved very satisfactory in the bad weather we have had lately, and they are very readily moved; and I think, according to Mr. Crossley's account, we may consider that his dome also possesses these advantages.

Mr. Newall. I have a dome 40 feet in diameter, with a telescope of 30 feet focus. It is not constantly used, but the wheels are constantly oiled. Unless the 12 wheels on which it rotates and revolves are well oiled, it takes rather more time than when it is in first-rate order to turn it. The walls are of galvanized iron $\frac{1}{4}$ inch thick. I deprecate very much the use of brick and stone in buildings of this kind. I keep the temperature inside nearly the same as the temperature outside, which I consider a great advantage. I hate hot water near the telescope; I think it rather tends to destroy definition. I have got no tins to catch the water. I left it open about 15 feet, and we had a gale of wind last week enough to blow the dome off, but I have got accustomed to it. I think brick or stone should not be near a dome of that kind.

Prof. Herschel. As a contrast in point of size to the dome mentioned just now, it will perhaps be interesting for me to mention that the Rev. Mr. J. M. Perry, the vicar of St. Paul's at Alnwick in Northumberland, has the late Mr. Cooper Key's 18-inch reflecting telescope set up at his house there, in the same little domed wooden hut or building which its constructor and former possessor, the late Rev. Mr. Cooper Key, at Hereford, provided as an observatory to contain it, and I do not think the diameter of the dome is greater than 15 feet. It is formed somewhat conically of wood and canvas, sloping up to a short ridge beam at the top, which is the upper bar of the frame of a pair of straight folding shutters. To see an 18-inch mirror housed in that way in a little hut scarcely fifteen or eighteen feet in diameter is in remarkable contrast to a dome about thirty-eight feet in diameter. The mounting of the late Mr. Cooper Key's telescope is a very remarkable one. It is mounted by securing the centre of the tube rigidly to a pair of iron legs which pass from the sides of the tube to a cross bar on the head of the polar axis, and which are then pivoted as straight-levers on the ends of the cross bar, projecting beyond it, one on each side of the polar axis, and have very heavy counterpoises on their projecting ends, so that the telescope is borne by its centre on a pair of straight levers whose heavy weights continually surround the polar axis on the one side and on the other, in whatever position of hour-angle or of right-ascension round the axis the telescope may be placed; while it can also receive the declination motion by the side-rods' or levers' motions round their pivots. It is thus a perfect equatoreal mounting and the most compact in form for a large reflector that I have ever seen.

Mr. Knobel. Is not that Mr. Berthon's principle of mounting?

Prof. Herschel. Mr. Perry has mentioned the inventor's name to me, which I think I remember now was Mr. Berthon.

Col. Tupman. Is it steady?

Prof. Herschel. Yes; remarkably steady.

Mr. Ranyard. The Rev. T. W. Webb had a Berthon mounting; it was rather smaller but it worked comparatively well. With regard to what Mr. Crossley has said as to hot water, I should like to hear what Mr. Common has to say on the subject. It is certainly important to preserve the instrument in damp weather, and I think that the danger of its spoiling the definition has been rather exaggerated. Since visiting Mr. Isaac Roberts my ideas have been changed; he has a dome connected with a room with a fire-place, which he uses as a sitting-room, and keeps at an ordinary temperature, and certainly his photographs show that the definition is good, and is not interfered with by the circulation of air through the shutters. That is a matter to be experimented upon with a large shutter; I expect that the danger is thought to be more than it really is.

Capt. Noble. I think that I can, to some extent, corroborate what Mr. Ranyard has said about heating an observatory. A few years ago I was visiting Mr. Rand Capron's Observatory at Guildford. He has a regular heating-apparatus in his computing-room and laboratory, which is in absolute communication with the part containing the telescope, there being a large opening between that and the dome; and I had as good a view of Jupiter through his equatoreal, with this heating-apparatus in action quite near (within 16 feet), as ever I have enjoyed in my life.

The President. I am inclined to agree with what appears to be the more modern view. You must consider the comfort of the instrument and of the observer as well as the definition you may get at night. If the observing-room be left without any artificial heating in the daytime, there is apt to be a deposition of moisture which prevents observation entirely, and it is better to be able to see something, even if there be a slight sacrifice of definition. At Greenwich, whenever a thaw sets in, the observer finds the object-glass covered with moisture and the microscopes illegible, so that he can sometimes do no work at all. The view that the room should be kept at such a temperature as to prevent deposition of moisture is a sound one, and has been adopted at the Cambridge Observatory, where the Transit-Circle room is heated with hot water; and at Greenwich we have arranged for heating the two principal domes with hot air. Hot water was not available, but it would be better than air, because with heated air you are liable to get sulphurous fumes which may injure the brass work of the instrument.

Mr. Spitta. I have tried hot-water pipes in my little shed which covers the 10-inch reflector and the 6½-inch refractor, having previously experimented with oil. It does not matter so much what is the heating agent, the real thing with pipes is the relation the heating surface bears to the cubical content of the observatory. The use of gas to heat the hot-water pipes is exceedingly convenient to the amateur observer—mine has been going the whole winter—and I have never found the pipes, if not unreasonably hot, interfere in the least with definition; but my experience is limited to one year.

Mr. Common. Mr. Crossley is right in saying that this paper is better adapted for careful reading than for discussion here. There are several points about the whole thing that I should like to make an observation or two upon. I have been in the dome, and I have seen the working of the shutter and the lifting platform of the dome; it answers the purpose of forming a shutter, which I felt the need of when I was using the telescope. My object in designing the old house was to get what was necessary at a certain cost, and I had to cut my coat according to my cloth. When I was designing it I felt that the wind might have very great effect upon the upper part of the tube and cause unsteadiness in the working, for though one can observe with the eye in a stiff gale, one cannot do anything then with photography. (Mr. Common then showed on the blackboard a form of shelter he suggested, consisting of a part of a dome only.) Of course this form of construction of telescope is most extravagant for a dome. There are a lot of valuable points about Mr. Crossley's arrangements worthy of the attention of those who are going to mount a telescope. As to the hot-water system, I think that, or its equivalent, is almost a necessity, and no harm can come either as to the definition or anything else, if proper care is taken not to use it too near the time when the telescope is to be used.

The President. It strikes me that Mr. Common's suggestion is a very practical one, and that it would save a great deal of weight in the dome and get over some of the difficulties in determining it.

Col. Tupman proceeded to read a paper by *Mr. Thackeray* entitled "Discussion of Greenwich Observations of North Polar Distances with reference to the position of the Ecliptic and an annual variation in the value of the co-latitude." Col. Tupman observed that the paper was of an exceedingly technical character, but it was also original, and the author had thrown an entirely new light upon a very important subject which would no doubt be most interesting to professional astronomers. To determine the true temperature of the air for the purpose of astronomical refraction always had been and always would be a matter of difficulty. The thermometer used for this purpose at Greenwich often differed considerably from the Meteorological Standard in the south ground, occasionally as much as 10°. Mr. Thackeray had investigated the effects of substituting the readings of the Meteorological Standard for the thermometer usually read in all the observations of Polaris and of the Sun for a long period of years. The ecliptic investigation showed that the place of the Equinox in longitude and the obliquity were not sensibly affected, but that the Sun's mean N.P.D. required the correction —0″·23, whereas the original ecliptic investigation gave +0″·22 for this quantity (1877 to 1886).

The observations of Polaris from 1851 to 1858 and from 1877 to 1886 are then arranged in monthly groups (expressed in seconds of co-latitude), and it is here that Mr. Thackeray brings out his interesting result, viz., all apparent variation in the co-latitude depending on the time of year. In the case of Polaris the extreme variation is no less than 0″·7 = a sixty-fourth part of the entire refraction! The results are compared with curves of temperature and moisture. The following are the final discordances in the case of Polaris, arranged according to temperature:—

Temp.	Discordance of Co-lat.	No. of obs.
°	″	
25	—·20	91
30	—·14	117
34	—·14	249
38	—·18	393
42	—·21	402

Temp.	Discordance of Co-lat.	No. of obs.
°	″	
46	−·09	436
50	−·08	437
54	−·04	257
58	+·06	274
62	+·16	274
67	+·30	276
74	+·52	228

The President. Mr. Thackeray, would you like to make any remarks? Are there any points you would like to mention?

Mr. Thackeray. I beg to thank the Secretary for so kindly reading my paper, and if I may be allowed I should prefer to make a few remarks after any discussion which may ensue.

Mr. Stone. The paper, so far as I could see in the short time in which I had the opportunity of looking it through before the Meeting, appears to me to be an exceedingly interesting one, and I think the results given in the tables are exceedingly valuable. With regard to the real difference between the thermometer used for the reduction of the observations, and what is called the standard thermometer of the observatory, I must confess I do not attach the same importance to it that a good many people do. We know of course that people are continually trying to get what they call the true temperature of the air—well I really do not quite know what they mean, for the reading of a thermometer will certainly to some extent vary with the circumstances of its exposure; but I think that in estimating the effects of temperature upon refraction it is exceedingly desirable that we should make the results to a very great extent depend on the temperature of the observing-room, as containing the last layers of air through which the rays have to pass. One point is sometimes, I think, overlooked. Wherever the thermometer is placed, if you determine the constant of mean refraction with readings of that thermometer,. then your refractions will in mean results represent your observations; and, so long as you proceed in this way, the absolute temperature of the air is not very important in question of mean refraction. But independently of the coefficient, which I call the coefficient of mean refraction, there is another constant which enters into our computation of the lobular refractions depending upon the difference between the temperature at the time of observation and the mean temperature; and, if the theory of refraction is incomplete, as is the case, this coefficient can be legitimately deduced also from a discussion of observations. It appears to me that the result which Mr. Thackeray has obtained shows that if this coefficient is determined from a discussion of the Greenwich observations with the present exposure of the thermometers that the coefficient thus found will sensibly differ from that adopted in the construction of the refraction tables in use, and this result appears to me important; and the amount of the discordance, as exhibited in Mr. Thackeray's Table, is certainly larger than I should have expected.

Except to show that this discordance is not got rid of by using the standard thermometer instead of that in regular use for the astronomical observations, I cannot regard the mere fact that different refractions would be required to represent the observations when the standard thermometer is used instead of the north thermometer as important. If the differences of the thermometer exposure leads to differences of thermometer readings, such must of course be the case.

I have seen many illustrations of such effects in practical work. Some years ago the coefficient of mean refraction was altered at Greenwich to avoid the necessity of some breaks of continuity in the refractions used, which had been found to represent the observations at considerable zenith distances.

This object was effected. But these breaks of continuity can otherwise be removed by slight adjustments of a second disposal constant which enters into Bessel's results; and it is therefore at least doubtful whether the adjustment should be made by alterations of the coefficient of mean refraction or not, or in what proportion the adjustment should be distributed between the two constants thus available for making the necessary adjustment. Whilst at the Cape I found the same running away of Bessel's refractions at considerable zenith distances; but when the thermometer readings on a crib in front of a south window of the transit-circle room were adopted for the computation of the refractions, the agreement between the Cape N.P.D. and the Greenwich N.P.D. reduced with Bessel's tables unaltered was very close indeed for any moderate zenith distances; and I therefore published the Cape results with refractions based on Bessel's tables unaltered and the temperatures of the crib. The agreement between the Greenwich and Cape results was noticed by other astronomers; and this agreement was regarded as a proof that the constant of mean refraction adopted by Bessel was sensibly accurate, and the changes introduced at Greenwich undesirable. To this decision I have no objection to offer, for the plan now adopted at Greenwich is that which I followed at the Cape. But it is necessary to remember that the agreement between the Cape and Greenwich N.P.D., upon which this opinion rests, depends upon the acceptance of the temperatures of the thermometer as then exposed in the crib. Recently, rightly or wrongly, I will not say, the use of the thermometer in the crib for the computation of the refractions has been abandoned at the Cape; and the readings of thermometers exposed in Stevenson's stands in different parts of the ground have been used. As the readings of the thermometers in these stands are known to differ not inconsiderably from the corresponding readings in the crib, it must follow that the Cape results reduced with Bessel's constants and the readings of these thermometers will differ from those which would have been obtained with thermometers in the crib and with the Greenwich results, unless some alterations are again introduced in the computation of the refractions at one or both of the stations to undo the effects produced by changes on the exposure of the thermometers. The results of recent Cape observations reduced with the new exposure of the thermometers show quite distinctly the effects of the changes thus introduced; but I am exceedingly doubtful of the expediency of continual changes in the exposure of the thermometers used for the computation of the refraction corrections, followed as they must be with alterations of the constants of mean refraction.

The effects of changes of refraction for changes in the exposure of the thermometers used in the calculation of the refraction appears to me therefore to possess no novelty; but the magnitude of the variations of the co-latitude of Greenwich as dependent upon the time of year or temperature appears to me an exceedingly interesting and practically valuable result.

Gen. Tennant. I should like to say a few words upon some of these points. The foundation on which all the theory of refraction more or less depends is that the strata of air of varying density are parallel to the surfaces of the Earth. Of course those mainly efficient are the lowest strata of air, and one of the most important things is that the strata of air in the observing-room shall be as nearly as possible parallel to the strata of air outside. But there is another point. I have very great doubt as to whether any thermometer whatever which you put into the cage or into any position of that sort really shows you the temperature of the air. I used a good chemical thermometer, one of which I knew the errors; I attached it to a string and found that, in a very short time, I got a temperature which, even under abnormal circumstances, was much more satisfactory than the ordinary temperature you can get by means of a stationary thermometer, and I have a great belief that a thermometer ought to be kept in motion so as to come into contact with a large portion of the air round it and not be kept so much in contact with certain layers and subject so much to the radiation of objects about it. You can easily understand that if you keep a bulb in motion in the air it meets with a great mass of air and takes up the temperature of the air, and that the average temperature of the air overcomes the neighbouring radiations; whereas if you keep it still, the temperature shown by the thermometer largely depends upon radiation from neighbouring objects; and I have always thought that this circumstance ought to be taken into consideration, and that it is necessary to make some change in practice for getting accurate results as regards temperature.

The President. There are two very important points raised in the paper of Mr. Thackeray and they require almost separate discussion. There is first the question of attempting to get the true temperature of the air, which is to a certain extent a conventional term; and, secondly, the effect of humidity on the refraction. I think Mr. Stone has overlooked one circumstance in this case, and he is rather under a misapprehension as to what is the intention at Greenwich. There is no idea of altering the system of ascertaining temperature, that is, the exposure of the thermometer to be used in the ordinary observations. I am quite at one with Mr. Stone there, but the difficulty we meet with here is that we are putting a thermometer on the north side of the building and we are trying to get a temperature which applies to the rays passing on the south side. Now at night there is no difficulty I think about that. It does not very much matter whether a thermometer is put on the north side of a building or on the south side at night; but there is a want of symmetry about the distribution of temperature in the daytime, and a thermometer exposed on the north side in the daytime will read much lower than one on the south side, because of radiation effects from the wall; so that in the daytime it seems better to use a thermometer freely exposed and which would represent more nearly the temperature of the south side. For observations of Polaris and of northern stars we still retain the ordinary exterior thermometer, and for all observations made at night. It is very desirable to have comparisons made between thermometers with different kinds of exposure. We have also compared those already mentioned with thermometers slung in the shutter-openings of the transit-circle to determine whether the strata of temperature within the room are horizontal or whether they are approximately semicircular. The whole question is difficult, and it seems to show that astronomers must become meteorologists if they want to deduce accurate fundamental North Polar Distances.

Mr. Stone said that with regard to the general principles stated he quite agreed. They must endeavour to get the temperature in the observing-room pretty much the same as the temperature outside, but he thought the temperature in the room must necessarily be used. No doubt for anything like perfect observation the temperature in the room ought to be kept the same as that indicated by a fairly exposed thermometer without; that would be perfect. But if they could not get that he was afraid the temperature in the room where the rays passed into the telescope would be the best temperature in the air available for use in refraction computations.

The President. There is this limitation to Mr. Stone's condition that the temperature of the room should be used. If we are to use the temperature of the external thermometer, the strata of air should be semicircular, about the centre of the instrument, and then the rays fall perpendicularly on the strata near the instrument and there is no refraction. But outside where the thermometer is exposed they begin to be horizontal, and it is rather a difficult point to define where we go from the one to the other;

but I think I am quite at one with Mr. Stone that you ought to have the temperatures inside and out as nearly equal as possible, and the more open the room is the better. I am inclined to think that the form of the Greenwich Transit-Circle room is rather unfortunate, as it is of a somewhat rectangular shape, whereas I should imagine the semicircular shape would be more suited for the determination of the temperature as affecting the refraction.

Mr. Downing. It is some consolation at all events that some such inequality as this is seen in every series of zenith-distance observations that I have examined, except perhaps those made at Pulkowa. And there they use a system of refractions constructed especially for the locality, which I venture to think ought to be done in every observatory which professes to give independent results. At Pulkowa also, for objects below 79° Z.D., they introduce a term into their refraction formula of the form $af(z)$, where the value of a varies from month to month, ranging from +0·3 to −0·3 throughout the year, and thus, for low objects, get rid of an inequality such as has been pointed out by Mr. Thackeray.

Mr. Thackeray. The principal reason I had in view in setting up one thermometer in judgment as it were on another, was not to try to prove that a thermometer in one position gave more accurate indications than one in another position, but simply to show what sort of effect a thermometer in one place or another might have upon objects like the Sun and pole star, which are observed throughout the year. In the case of the Sun the difference of readings between the meteorological standard and the ordinary thermometer used in the reductions gives a mean correction amounting to nearly ″·3, but in the case of the pole star this mean correction is only ″·02, while the mean monthly corrections range from +″·3 to −″·1. If this can be said to prove anything it might possibly be this, that a star observed at all hours throughout the year was practically independent of the position of the thermometer with which the refraction was reduced, but that the position of a star-object observed at particular times of the day or year were peculiarly liable to feel the effects of any peculiarity in the position of the thermometer. In all cases the observations of the Sun have only been taken as a rough check on the results of the observations of the pole star. There is another point, and it is this: the annual variation in the values of the co-latitude given by the observations of the pole star resolves itself into discordances between the annual mean and the monthly means of the above and below pole observations, which follow very closely the curve of temperature corresponding to the time of transit, or the curve of differences between the readings of the wet and dry bulb thermometers: if further we arrange these discordances in terms of temperature alone, thereby collecting together observations from widely different times of the year except in the case of very high or low temperatures, we get the systematic discordances you see; but whether they are due to a defect in the temperature correction itself, or to the absence of a secondary correction for humidity there appears to be no certain indication. There is another point I should like to notice. In the 'Monthly Notices,' Vol. xl., you will find a paper in which Mr. Stone compares the star-places common to his Cape Catalogue, 1880, and the 'Nautical Almanac,' 1880; and the discordances in N.P.D. depending on R.A. he gives as follows:

0^h–6^h.	6^h–12^h.	12^h–18^h.	18^h–24^h.
$-''.55$	$-''.42$	$+''.08$	$-''.25$

Making the following rough assumption that stars on the equator are observed at Greenwich and the Cape at 10 P.M. and 11 P.M. respectively, every night throughout the year, I find the computed discordances from this formula—

$$\frac{\text{Refraction}}{\text{Refraction of Polaris}} \times \left\{ a(t-55)+b(t-55)^2 \right\}$$

where the values of a and b are supplied from the present investigation, and the values of t from the best available sources—is as follows:

0^h–6^h.	6^h–12^h.	12^h–18^h.	18^h–24^h.
$-''.47$	$-''.57$	$-''.02$	$+''.09$

A vote of thanks to the author of the paper was passed.

Mr. Ranyard. I have a note, which will not take more than a minute to read; it is on a simple method of applying electrical control to the driving-clock of an equatoreal telescope. It is a matter to which a good deal of attention is being directed at present, and I think it worth while to mention the matter to the Meeting. I am having a large driving-sector of 3-feet radius made, and I propose to control the driving electrically and to get rid of the epicycloidal wheels used by Sir Howard Grubb in the clock he showed at the last meeting. I make use of the instantaneous current, as in Dr. Gill's and Lord Crawford's clock as well as Sir Howard Grubb's, only they require a further apparatus to turn on a current which lasts during the whole remainder of the second. I have an insulated wheel very similar to Lord Crawford's, and the current from this passes into horseshoe electro-magnets, which control a lever that moves backwards and forwards over the fulcrum of the crutch which pinches the revolving disc of the governor. My governor is similar to the one used by Mr. Common and figured in the 'Memoirs' in the description of the mounting of his 37-inch reflector. When the weight of the lever is on the inside of the fulcrum it depresses the crutch and allows the revolving-disc to go faster; when on the other side it increases the pressure of the crutch. When the clock is going exactly as it should with the pendulum, the current passes into a third wire and the lever is drawn to the middle position over the fulcrum, and its weight produces no effect.

The following papers were announced:—

J. Tebbutt. "Observations of the Occultation of Venus by the Moon, 1888, March 9."

E. Nevill. "Observations of Stars observed during the Lunar Eclipse of 1888, Jan. 28."

A. Graham. "Observations of Sappho made at the Cambridge Observatory with the Northumberland Equatoreal and square-bar Micrometer."

A. B. Biggs. "Observations of Comet *a* (Sawerthal) 1888 made in February and March at Launceston, Tasmania."

Capt. R. Belding. "Sextant Observations of Comet *a* 1888, extracted from the meteorological log kept on board the barque 'Atlantic.'"

Prof. J. A. C. Oudemans. "On the condition that in a Double-image Micrometer the value of a revolution of the Micrometer-screw be independent of the accommodation of the eye."

Royal Observatory, Greenwich. "Observations of the Spectrum of Comet 1888 *a* (Sawerthal)." (Communicated by the Astronomer Royal.)

Radcliffe Observatory, Oxford. "Observations of Comet 1888 (Sawerthal)." (Communicated by Mr. Stone.)

Royal Observatory, Greenwich. "Observations of Comet 1888 *a* (Sawerthal)." (Communicated by the Astronomer Royal.)

W. G. Thackeray. "A discussion of Greenwich Observations of North Polar distance with reference to the position of the Ecliptic, and an annual variation in the value of co-latitude."

A. M. W. Downing. "The positions for 1750·0 and proper motions of 154 Stars south of 29° declination, deduced from a revision of Powalky's reduction of the Star-places of Lacaille's Astronomiæ Fundamenta."

Prof. J. C. Adams. "Remarks on Sir Geo. Airy's Numerical Lunar Theory."

A. C. Ranyard. "Note on a simple method of applying Electrical Control to the Driving-clock of an Equatoreal."

J. R. Hind. "Note on the Total Solar Eclipse of 1889, December 21–22."

Thomas Dallmeyer, Bloomsbury Street, W.C.; *Alfred Robert Johnson, M.A.*, St. John's College, Cambridge; *Reginald Carter Nelson*, 19 Roker Terrace, Sunderland; *Sir Henry Thompson, M.B.*, 35 Wimpole Street, W.; *John Walther, M.D., C.M.Edin.*, West House, Eastbourne, were duly elected Fellows of the Society.

The following gentlemen were proposed for election as Fellows of the Society:—

F. Bolton, Calcutta. (Proposed by R. Wigglesworth.)

A. C. de la Cherois Crommelin, B.A. (Proposed by Colonel Tupman.)

A. E. Leahy, M.A. (Proposed by H. H. Turner.)

S. M. Baird Gemmill. (Proposed by Rev. A. Henderson.)

Meeting of the Royal Meteorological Society.

Wednesday, May 16, 1888.

Dr. W. Marcet, F.R.S., *President*, in the Chair.

Mr. L. W. Gatward and Mr. N. Simmons were elected Fellows, and Prof. D. Colladon an Honorary Member, of the Society.

The following communications were read:—

1. "Report of the Wind-Force Committee on Experiments with Anemometers conducted at Hersham," by Mr. G. M. Whipple, B.Sc., and Mr. W. H. Dines, B.A. A whirling apparatus, with arms 29 ft. radius, was rotated by means of a small steam-engine. On the arms of the whirler four different anemometers were placed. Each experiment lasted 15 minutes, the steam-pressure remaining constant during the run. For the Kew Standard Anemometer, with arms 2 ft. long, the experiments give a mean value for Robinson's factor of 2·15, and for two smaller instruments the factor is 2·51 and 2·96. Mr. Dines's Helicoid Anemometer gives very satisfactory results, the mean factor being 0·996.

2. "On the Measurement of the Increase of Humidity in Rooms by the Emission of Steam from the so-called Bronchitis-Kettle," by Dr. W. Marcet, F.R.S. The author described a number of experiments which he had made by steaming a room with a bronchitis-kettle, and ascertaining the rise and fall of the relative humidity from readings of the dry- and wet-bulb thermometers. He found that the air in the room could not be saturated, the relative humidity not exceeding 85 per cent.

The Photographic Chart of the Heavens.

Mon cher M. Common,—

En attendant que le comité permanent s'occupe de la meilleure méthode à employer pour utiliser les clichés de la carte du ciel afin d'en conclure les positions des 2 millions d'étoiles qui entreront dans le catalogue, je crois devoir répondre quelques mots à la note que vous venez de publier dans l'*Observatory* relativement au projet de mesure présenté par le Dr. Gill. Permettez moi de vous dire que je vous trouve bien sévère pour un projet aussi bien étudié et venant d'un savant aussi habile et compétent que le Directeur de l'Observatoire du Cap. Il y a certainement quelques exagérations dans les termes dont vous le blâmez au point de vue des dépenses.

Il est cependant bien évident que le Congrès International en votant la construction par la photographie d'un catalogue de haute précision, a bien dû prevoir qu'il faudrait faire par les clichés de très nombreuses et très délicates mesures pour en tirer les éléments

du catalogue et que ces mesures faites pour 2 millions d'étoiles couteraient beaucoup d'argent et de temps. Actuellement les astronomes admettent d'après une longue expérience que l'observation et le calcul d'une étoile revient environ à 10 francs, et pour l'Observatoire de Paris ce chiffre serait même plutôt trop faible; les 2 millions d'étoiles couteraient donc 20 millions de francs, et deux siècles de travail peut-être.

Pour la photographie et le projet de mesures du Dr. Gill cela ne reviendrait qu'à 6 millions et une vingtaine d'années; il trouve même dans son mémoire en tenant compte de divers éléments que l'économie sur la dépense serait plus grande encore et que leur rapport serait de $\frac{1}{8}$ au lieu de $\frac{1}{3}$.

Si ce projet n'était pas adopté il faudrait que vous et que vos savants collègues de la Société Royale qui le refusent nous en montrent un meilleur, c'est à dire qui soit moins couteux qui exige moins de temps et qui assure toute la haute précision de mesures que permet d'espérer l'organisation d'un bureau central et qu'a si fortement demandée le congrès international.

Si chaque observatoire devait procéder lui même à des mesures, ce serait certainement plus long tout aussi couteux et on serait loin d'avoir la garantie d'exactitude que donnerait la centralisation de mesures par le même personnel spécialement créé et organisé pour ce travail.

Il est bien nécessaire aujourd'hui que tous les astronomes qui s'interessent à cette très importante question, y reflêchissent mûrement, qu'ils ne se bornent pas à critiquer ou refuser les projets presentés mais qu'ils émettent aussi leur opinion personnel sur la meilleure manière d'utiliser nos clichés au double point de vue de l'économie de temps et d'argent et de l'exactitude.

E. MOUCHEZ.

GENTLEMEN,—

Your article on "the Photographic Chart of the Heavens," which appears in the 'Observatory,' No. 136, would seem to have been written, in one important respect, under some misconception as to what was really discussed at the recent Paris Congress. Fortunately the published minutes of that Congress are sufficiently clear upon the point, and I would beg to call your attention to the facts relating to the 'Catalogue of Stars,' upon which you animadvert so strongly.

It having been proposed by the Technical Committee that a Photographic Chart of the Heavens should be made to include stars of the 14th magnitude, it was pointed out by M. Fizeau (p. 42) that the brighter stars would be so over-exposed that another series of plates should be undertaken with a shorter exposure. This was the first suggestion of the second series of plates.

In the Astronomical section (p. 61) it was suggested that plates for the chart should be taken with a "réseau" photographed on them; but the objection was raised by Dr. Schönfeld (p. 62) that

the "réseau" would obliterate too many faint stars; and eventually (p. 64) it was suggested that the "réseau" be on a second series of plates of short exposure.

The catalogue of stars to the 11th magnitude, the suggestion for which you erroneously attribute to a recent paper by Dr. Gill, will be found first mentioned on p. 66 of the minutes (April 21). M. Cornu put the point clearly before the meeting in these words —"It is now necessary to decide whether a catalogue should be added to the chart (of the heavens to the 14th magnitude). A general catalogue would be impossible; it is therefore necessary to limit the question and to restrict it to stars of the 10th or 11th magnitude." On a subsequent day (p. 24), Dr. Schönfeld pointed out that it would be hardly worth while to make a catalogue only to the 10th magnitude, and he estimated that down to the 11·5 magnitude there would be about 3¼ millions of stars in the heavens.

From the discussion that will be found on pp. 69, 70, and 23, there cannot be the least doubt that the word "Catalogue," in resolutions 18 and 19 voted by the Congress, means a catalogue of all stars to the 11th magnitude—probably some two millions. Those who voted against these resolutions (among whom I was one) did so, I believe, on the ground that they pledged the Congress to work not foreshadowed in the original programme, and which was of so extensive a nature as to make it too problematical of accomplishment. But whether such a catalogue be "the most utter waste of time, labour, and money that it could enter the mind of man to conceive," is a strong expression which a perusal of the Congress minutes shows is not shared by many astronomers whose opinions are entitled to the greatest respect.

Yours faithfully,
Edw. B. Knobel.

Bocking, Braintree.
1888, May 15.

We print above letters from Admiral Mouchez and from Mr. Knobel, concerning the remarks we made last month on Dr. Gill's proposition to catalogue two million stars. There is a somewhat personal implication in both letters, to which we must at once reply before proceeding to treat of the real question at issue—a suggestion that we have been so emphatic in our disapproval of the scheme as to be discourteous to its supporters. We may perhaps venture to doubt whether either writer has done us the honour to read our remarks carefully enough. Admiral Mouchez "nous trouve bien sévère pour un projet aussi bien étudié et venant d'un savant aussi habile et compétent que le Directeur de l'Observatoire du Cap." We have not said a single word in disparagement of the skill and care with which Dr. Gill's paper has been written; we have vehemently objected to the question being raised at all; and that we have objected so vehemently may be taken as a full recognition of Dr. Gill's prominent position, which makes it a matter of necessity to bring all our forces to bear against a scheme which he chooses to advance. Mr. Knobel is perhaps more unjust to us. We have not in an unqualified manner characterized a catalogue of 2,000,000 stars as "an utter waste of time, labour, and money"; but we did use even stronger language about cataloguing stars "for the purpose only of getting their places written down," in order to call attention to the '*reductio ad absurdum*' of cataloguing towards which we very much fear there is some apparent tendency. And, finally, if we have been so emphatic as to be accused of exaggeration, let us again point out that a scheme, which we contend has not been assented to or even considered by the members of the Astrophotographic Conference, has been quietly launched, and is now so far under way that it is referred to by the President in the opening sentence of his letter as a matter already accepted by the 'Comité permanent,' and as only remaining to be discussed in detail. Surely it is time for those who have the welfare of the scheme really sanctioned by the Conference to raise their voices loudly in protest!

So much in explanation of the tone we have adopted in speaking of this proposal, and we now return to the letters. The main point of both is that this scheme of a catalogue of 2,000,000 stars has not been originated by Dr. Gill, but was really considered and approved by the Conference. As we have stated above, we hold the opposite opinion,—that although two resolutions of the Conference do mention *a* catalogue, this term cannot be supposed to sanction a catalogue of 2,000,000 stars without further specification. The Conference met to discuss the advisability of making a chart. With the invitations sent out to the various astronomers to attend this Conference there was sent a "programme provisoire" (which, it is to be very much regretted, was not that considered by the Congress). This first "programme provisoire" was dropped, and at the first séance of the Congress another was produced. In the first, in article 19 a *catalogue of reference stars* was mentioned, and properly so, but in the second there was no mention of any such catalogue. Mention was made in section 4 of a means of publishing the chart and the form of publication, but up to this time there was absolutely no question before the Conference of publication of a catalogue either of two million or any other number of stars. There was no doubt a feeling amongst some astronomers present that a catalogue would be as useful, in their judgment, as the chart; and they took the opportunity of putting forward their views when the question of a second series of plates was brought forward. The taking of this second series of plates was proposed to meet an anticipated difficulty in photographing parts of the heavens where the stars differed greatly in magnitude. It was decided (Resolution 17) that a second series of plates should be taken, in order to ensure the greatest precision in the micrometrical measurement of the stars of reference, and to render possible the construction of a catalogue. Here we have the first mention of a catalogue in the resolutions noted. A reference to the minutes of the Congress will show that this resolution was a compromise, for there had already been before the Congress a direct proposition (that of M. Tacchini) for a catalogue, which, however, was not voted upon. The resolution was in fact an endeavour to settle a question that was before the Congress, viz., whether the plates should be so taken as to be capable of accurate measurement; and this is decided by the specification that they shall render possible the construction of a catalogue. The next two resolutions speak of the second series of plates as *destinés à la construction du catalogue*, but nowhere is any direct resolution to be found as to the construction of a catalogue of all the stars.

If these resolutions need interpretation by the light of subsequent consideration at all, we may suggest a very different direction in which they might be modified in actual fact, and in which their spirit would yet be even better represented than by a literal fulfilment. It was pointed out that in taking the photographic plates of stars down to the 14th magnitude in parts of the sky where brighter stars existed, these with the exposure necessary to obtain the 14th magnitude would be very much over-exposed. And it was suggested that it would be advisable to take a second series of plates, as already mentioned (see Resolution 17). Now in some parts of the sky no second series of plates are, from this point of view, at all necessary; whilst in others not one or two, but many series of plates would be necessary in order to do justice to the various magnitudes in that particular part of the sky. For the present this is not the point at issue, but it may serve as an illustration of the sort of interpretation of the resolutions which we should consider legitimate.

In order to come to a proper judgment on the legitimacy of the derivation of Dr. Gill's proposal from the resolutions it is necessary to make some statements, which are not new, but of which the true significance does not seem to have been universally appreciated:—(1) When the plates are obtained they are actual representations of the stars as existing at a given time, and for every purpose except spectrum analysis are as good, if not better, than the visible heavens. If with these plates we have the absolute places of a certain small number of known stars, we have then all the data to make them valuable, either in the present or in the future. (2) The many questions concerning the stars which it is hoped a photographic chart of the heavens would do a great deal towards settling, such as their distribution, their proper motions, their changes of magnitude, and the presence of minor planets, of new stars and the like, can all be best treated by a direct comparison of plate with plate, in any of the various ways in which this can be done. (3) In order to obtain the best results from such an agent as photography it is necessary to use it in its own proper way; and astronomers must recollect that old methods of procedure adapted to other instrumental means may most probably be out of place. We might considerably enlarge on these statements, but for our present purpose it is sufficient to call attention to them.

Now, if Dr. Gill's catalogue were successfully constructed—and there are, alas! many difficulties in the way—its utility in the direction of comparison of our sky with that of the future is wholly limited by one condition, that in the future another exactly similar catalogue be constructed, occupying a similar time. Even then, if any changes were found by means of this comparison of catalogues which might very well be made in the course of fifty or one hundred years, the natural and indeed the proper thing to do would be to immediately compare the original plates. But can it be possible that any man or number of men really think of dealing with such a subject in such a way? If, on the other hand, the object of a catalogue be merely to allow of comets, minor planets, and other bodies being located, surely it would be better to measure the plates as occasion arises, and not to catalogue 2,000,000 stars on the off-chance of having some twenty or thirty positions to settle in the course of a year. And, further, such a catalogue would have this enormous disadvantage, that whilst in some parts of the sky stars of the eleventh magnitude would be fairly well spread, in the Milky Way we should have stars clustered in such enormous quantities that it would be an extremely difficult thing to even identify them: in fact, speaking roundly, we should say that if such a catalogue were made, two-thirds of the stars catalogued would lie in the Milky Way. If, contrary to the opinion we have expressed, it is decided to form a very large catalogue, surely it would be better to determine the places of a certain number of stars, of such magnitudes as are found available, in each square degree, and make these the reference stars from which the positions of the other stars on the plate could be obtained.

We are therefore of opinion that supposing limitless time and money available for such a purpose, the advantages of constructing this Catalogue would be doubtful; but even if we waived all these objections and agreed that such a catalogue would be a "nice thing to have," or admitted that since men of the ability and reputation of Admiral Mouchez and Dr. Gill consider such a catalogue necessary it is heresy to enquire the why and wherefore, there would still be left the serious objection that to form a chart of the heavens is the first thing to do, and, take it in as simple a form as possible, it will quite possibly tax the energies of astronomers to their utmost: and that stellar photography being as yet in its infancy it is suicidal to attempt anything which will commit us to a course of action extending over more than a very few years. We could not give a better illustration of the dangers of the opposite procedure than has been supplied by Admiral Mouchez himself. In a recent article he has suggested that there have lately been such improvements in the sensitiveness of plates that we could now go to the 15th magnitude instead of the 14th. With a little ingenuity and less arithmetic it could easily be shown that the whole plan of operations would have become hopelessly futile and obsolete before half the time allowed by Dr. Gill for its completion had elapsed.

But not for one moment do we wish to appear lacking in sympathy with those who have spent and are spending so much time and thought on this subject; it is our great anxiety for the success of the work in which they are cooperating which makes us eager to protest as far as we can against the grand mistake of attempting too much.

EDITORS.

Selenographical Notes.

ARISTILLUS AND AUTOLYCUS.—On the evening of April 18, between 8^h and $8^h\ 45^m$, the morning terminator almost exactly bisected Aristillus, only two or three lofty outlying sections of the ring on the north-east side being as yet in sunlight. At this stage, if the air is clear and tranquil, as it was on this occasion, so as to permit the employment of high powers, a view is obtained of the marvellous detail on the south-west, and lying between the ring-plain and its smaller neighbour Autolycus, which will well repay a prolonged examination. With the exception of Schmidt's large map, no existing representation with which I am acquainted gives more than a very general and superficial notion of this interesting region. We read of "gullies" and "lava-streams" as existing round about the *glacis*; but these terms inadequately, and to a great extent erroneously, describe the true surface-structure as revealed under a low sun by magnifying-powers of from 300 to 420. A glance at the formation under good conditions 24 hours or so after sunrise shows that the crowd of ridges, hillocks, and valleys which environ it are for the most part wanting on the south-east; and that on the south-west and north-east they do not, strictly speaking, radiate from the centre of Aristillus or from any other centre, but trend in nearly parallel rows from north-east to south-west, so that those on one flank have almost the appearance of being prolongations of those on the opposite side. On the north-west the arrangement is different, as is also the character of the features. Here a number of spurs and projections from the wall, which ultimately degrade into low ridges and mounds, curve out from the border (the curvature being uniformly convex towards the north-east) and run towards Theætetus, two or three of the longer ridges extending to the south-east side of this ring-plain. On the south-west we have as the most prominent object at sunrise a cleft-like valley abutting at its widest end, where it is at least two miles in breadth, on the outer terrace of the wall, and extending in a nearly straight line to a distance of 30 miles or more towards the south-west, and becoming gradually narrower and shallower in its progress. All the manifold scorings and surface-markings for some distance south-east of this maintain a strict parallelism to it, with the exception of two or three ridges which strike across the system at right angles. If these are the objects which some observers have described as "lava-streams," it must be confessed that the resemblance thereto (with high powers) at sunrise, if it exists at all, is not more striking here than in many other localities where similar features are found. They seem rather to be more or less deep parallel valleys and longitudinal ridges, which, except under a very low angle of illumination, when the associated shadows are pronounced, are by no means either dark or otherwise prominently distinguishable from the tone of the neighbouring surface. Schmidt's representation of these objects is worthy of all praise, and is evidently the result of careful observation under varied conditions; but he does not show a remarkable group of three coarse rows of inosculating craters radiating, not from Aristillus, but from a point near the foot of the *glacis* of the south-west wall, and spreading out like the sticks of a fan towards Autolycus, the component craters gradually diminishing in size as they approach this formation, at the foot of the north-west wall of which they terminate. Schmidt represents only the most westerly of these rows, but draws a line of parallel hillocks crossing them at an oblique angle, which was well seen on April 18.

Nearly due north of Aristillus and a few miles east of the great ridge which runs northwards from the formation to the bright mountain Piton, there is a curious low elliptical ring about 10 or 12 miles in diameter, which at sunrise, and for some time after the crest of the wall of Aristillus is fully illuminated, stands out conspicuously amid surrounding darkness. As sunrise progresses a small area within the ring is illumined, thus giving the formation all the appearance of a true ring-plain with a low central mountain. A fine view of this object was obtained on Jan. 31, 1887, 6^h, and on April 18, 1888, $8^h\ 20^m$. On the first-mentioned date, when sunrise was somewhat more advanced than on the last, a second ring, about one third the diameter of that in question, was visible on the east side of it. Schmidt is the only selenographer who shows this formation, within which he represents two parallel ridges traversing it from north-east to south-west and cutting through its north border. These I have not seen; but on May 18, when the east long. of the morning terminator was about 5° 30′, a small mountain was visible a little south of the centre, the ring being still perfectly traceable—indeed, it was just discernible in bright twilight on May 20, when the solar altitude was more than 30°.

On the north-east side of Aristillus, not far from the border, stands a circular mountain which, though small, cannot, judging from the length of its shadow, be much lower than the crest of the neighbouring wall. Its locality is a blank in all the maps.

Schroeter, however, shows it prominently (Sel. Top. Frag. T. lxx. fig. 37). On April 18, and as a fainter object on May 18, a delicate white streak was noted, extending from the south-east wall of Theætetus to the foot of the north-west wall of Aristillus. I strongly suspect that this is a narrow cleft, possibly forming a continuation of cleft θ, (S. 102) north of Theætetus.

There is a good-sized crater a little below the summit of the west wall of Autolycus which Mädler does not show, and which Neison draws much too far to the north of its true place and does not mention in the text. It is easily detected about the time when the morning terminator reaches the centre of Archimedes, at which stage it is prominent, but a few hours before or after this is it only seen with difficulty, if at all.

Kempston, Beds, 1888, May 21. THOMAS GWYN ELGER.

The Classification of the Heavenly Bodies.

THE theory which Mr. Lockyer put forward at the Meeting of the Royal Society on November 17 as to the meteoric origin and constitution of the heavenly bodies, has received a further and an important development in his Bakerian Lecture on April 12, under the title of "Suggestions on the Classification of the Various Species of Heavenly Bodies." Briefly the new classification proceeds on the principle of distinguishing the bodies with, supposed, increasing temperatures from those with decreasing temperatures, thus overcoming the objection often and justly made to the classification of Secchi and Vogel—that it represented only the processes of stellar decay, not of stellar growth. The groups formed are seven in number, and are as follows:—

Group I.—Spectrum with radiation-lines and flutings predominant. Absorption beginning in the last species. The constituent meteorites are as yet sparsely distributed. The group includes nebulæ, comets, and bright-line stars.

Group II.—Spectrum with mixed radiation and absorption predominant. The true star-stage not yet reached, but the constituent meteorites much more closely packed. The group corresponds to Secchi's third type of stars, or Vogel and Dunér's class III *a*.

Group III.—Line absorption predominant in the spectrum; temperature *increasing*. The various species marked by increasing simplicity of spectrum. The true star-stage is now reached, the group including certain stars of Secchi's second type or of Vogel's class II *a*. *a* Cygni may be taken as the representative of this class.

Group IV.—Spectrum with the simplest line absorption predominant. This group corresponds to Secchi's first type or Vogel's class I *a*. Sirius and Vega are typical representatives, and the group occupies the apex of the temperature-curve.

Group V. corresponds to Group III., but the temperature is *decreasing*, and the successive species of the group are marked by increasing complexity of spectrum. It comprises stars like our Sun and Arcturus, which have in the earlier classifications been grouped together with stars of the type of α Cygni. The appearance of the H and K lines has been suggested as forming a means of discriminating between Groups III. and V.,—when the lines are narrow the spectrum being included in Group III., when they are broad, in Group V. This is, however, necessarily but a tentative suggestion at present.

Group VI. shows a spectrum with carbon absorption predominant, and includes therefore the stars of Secchi's fourth type or Vogel's Class III *b*.

Group VII.—Luminosity extinct, the stage reached by planets.

The Bakerian Lecture was mainly occupied with the detailed discussion of the various species of bodies included in Groups I. and II. and the causes of their variability, and the way in which the details of the spectra of the bright-line stars and those of Secchi's third type are classified and explained is most ingenious and complete, and evidences a most painstaking and able discussion of the material available. For the third-type stars Dunér's great work has been the chief authority; and following out the idea brought forward in the November paper of the threefold character of this type of spectrum—viz. a continuous spectrum, an emission-spectrum of carbon, and an absorption-spectrum due to low temperature metallic flutings—Mr. Lockyer renders account of every variation in Dunér's observations. It is scarcely possible to speak too highly of the ingenuity and skill with which these explanations have been worked out, or of the analogous methods by which the bright-line stars have received the solution of their enigmatical spectra. There would be little left to ask of stars of these types if the theory here put forward could be at once accepted in its entirety.

Fortunately for the spirit of research there is very much yet to be done before Mr. Lockyer's theory, however ingenious, plausible, or even probable, can be regarded as established. It may be conceded—indeed the fact was generally accepted years ago—that the behaviour of *Novæ* indicates that these are not true stars, but collections of discrete particles, of which the individuals are comparatively small and separated by greater or less intervals, similar to the meteor-swarms which encounter our Earth. And the known connection of many Novæ with nebulæ may induce us to look favourably on a theory which gives nebulæ also more or less of a meteoric character. But it must not be forgotten that as yet the identification of the nebular lines with those given by meteorites has been only effected by low dispersions, and should therefore at present be considered as subject to correction. The evidence, for example, for considering the brightest nebular line as being the low temperature fluting of magnesium is not measurably greater than that which *once caused it to* be ascribed to nitrogen. Then, so far as yet appears, the sole cause of nebular luminosity in Mr. Lockyer's view appears to lie in the collisions of the members of different meteor-swarms. But it may certainly be doubted whether anything we yet know of the density of meteor-swarms and the size of their component members justifies the idea that such collisions could be, and could continue to be, for ages, the sole, or even the chief cause of nebular light. Could this be granted it would not not be difficult perhaps to accept Mr. Lockyer's further suggestions as to the origin and meaning of the different forms of nebulæ. The same may be said of the explanation of the variations noticed in so many stars of Group II. We know nothing by experience of the results which would be produced by the collision of two meteor-swarms; and Mr. Lockyer has made no effort to supply the defect of experience by mathematical reasoning. The bare suggestion is made, and it is freely treated as a proved and established fact, but no attempt is made to offer any numerical proof that it is at all competent to produce the desired effect.

The feeble dispersion which hitherto has been all that could be applied to the study of bright-line stars throws some amount of uncertainty on the very ingenious composition of spectra which Mr. Lockyer suggests for their explanation. A single point will illustrate this. We have in some of these spectra a bright line near D. This has usually been identified with D_3, the well-known "helium" line of the chromosphere, wave-length 5875. Mr. Lockyer suggests that it is rather the bright edge of a low temperature manganese fluting at 5870. The distance between the two lines is about that between the two D lines, yet at present we cannot assert with confidence with which of these two the stellar bright line coincides, if indeed it coincides with either. Yet the entire theory turns on this very point.

The same remark applies to the detailed study of the spectra of Group II., Secchi's third type. It is impossible for a spectroscopist to see the wonderful way in which Mr. Lockyer has classified Dunér's observations without pleasure and admiration, and without a strong inclination to accept a theory which brings into so striking a harmony such a mass of material. But it is evident that we have here rather the results of laboratory than of telescopic work; and admirable as Dunér's observations are, when we recollect the faintness of many of the objects he examined, and the comparative insufficiency of his equipment, there will be a reluctance to accept the reported absence of this or that band in a particular star as of any great significance. Then, again, the leading idea of the explanation of the spectra of this type, the presence of bright carbon bands, is by no means proved. There is undoubtedly an appearance as of a bright band coincident, or nearly so, with the brightest of the carbon spectrum—that in the green; but the case of Dr. Draper's reputed discovery of the bright lines of oxygen in the solar spectrum should teach us caution, and as yet the evidence for bright carbon in these stars is by no means so strong as that Dr. Draper marshalled in favour of his supposition.

On the whole, Mr. Lockyer's new classification is a distinct advance upon those which have preceded it; it has a breadth, harmony, and completeness which they have lacked. But the meteoric theory on which it is based stands before us at present, on its spectroscopic side at least, not as a chain of established facts, but as a plausible working hypothesis. As such it must hold the field for some time to come, and it will prove of the utmost service by giving a point and direction to the study of stellar physics which it has hitherto lacked. If stellar spectroscopy is to proceed at all, the next steps in its progress will have to be upon the lines which Mr. Lockyer's theory has laid down. E. W. MAUNDER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Moon's Libration and the 'Nautical Almanac.'

GENTLEMEN,—

In connection with Mr. Backhouse's letter in the 'Observatory,' No. 136, perhaps I may be permitted to inquire why the table of "Mean Time of the greatest Libration of the Moon's Apparent Disc" disappeared from the pages of the 'Nautical Almanac' after the year 1877? Faithfully yours,

WILLIAM NOBLE.

Marshalls Manor House, Maresfield,
Uckfield, 1888, May 12.

Tail of Comet Sawerthal.

GENTLEMEN,—

It is stated in the 'Observatory,' No. 136, p. 238, that on only one morning out of four on which this comet was observed at Greenwich in April was a tail visible, viz. on the 6th. There may possibly be some error in this statement. Here, on the morning of April 16, at 3^h 45^m, when comet was 5^h 30^m east of the meridian and in Dec. 16° 40′ N., the tail was well defined about $\frac{1}{8}$° from the nucleus, reaching to a star slightly greater in brightness than that; not visible to the naked eye, the twilight having commenced.

This morning (May 14), at 2^h 21^m, the tail stretched exactly across the field of a $2\frac{1}{4}$-in., power 70, and was therefore a little over $\frac{1}{2}$° in length. The extent seemed the same as on April 16, only it was very much fainter and not visible in an opera-glass. It was so sensitive to the least approach of dawn that it was not so distinct ten minutes afterwards in a $3\frac{1}{4}$-in., power 50. At 2^h 34^m it was just 6 hours from the meridian and in N. Dec. 23° 0′.

The comet was not perceptible here on any occasion with the naked eye. If such had been the case, it would be the seventh comet visible to the unaided vision within the last seven years, whereas during the twenty years preceding 1881 only one conspicuous comet and one faint one were so visible, and during the whole of the first half of the 17th century Arago only puts down two comets on his list.

Faithfully yours,

S. J. Johnson.

Melplash Vicarage, Dorset,
1888, May 14.

[The remark about failing to see the tail was intended to be descriptive of our morning weather, not of the comet itself; the tail has been seen on many nights since when the weather has been very much finer.—Eds.]

Encke's Comet.

Gentlemen,—

Now that another return of Encke's comet is almost due, it may be interesting to recur to the appearance at which it was first seen in the year 1786, when it was discovered near β Aquarii by M. Méchain at Paris, on the evening of January 17. It was so nearly setting that he could only obtain a single observation. Communicating his discovery the next day to M. Messier, the latter also looked for the comet in the evening, but the sky was unfortunately clouded until it was too late. Both astronomers observed it on the evening of the 19th, but after that several evenings were cloudy, and meanwhile the comet's southerly motion had rendered further observations impossible at Paris, the only place where any were obtained at that appearance. No observatories existed in the southern hemisphere until more than thirty years afterwards; and it is an interesting circumstance that at the first ever established, that of Sir Thomas Brisbane at Paramatta, all the observations of Encke's comet were made which were secured at its first *predicted* return in 1822.

The author of the article "Astronomy" in the ninth edition of the 'Encyclopædia Britannica' (vol. ii. p. 814) states that this comet was observed in 1789 and 1801; the former mistake is probably a *lapsus plumæ* for 1786, the latter is less easy to explain. The comet was not seen after 1786 until 1795, when it was first seen by Miss Caroline Herschel on the 7th of November, and stated by her brother to be just visible to the naked eye. It was observed both at Greenwich and Berlin until the 24th of November, and then not seen again until the 19th of October, 1805, when it was once more discovered, under the supposition of its being a new comet, by Thulis at Marseilles. Pons discovered it at the same place in 1818, soon after which its periodicity was detected and the length of the period determined by the astronomer whose name it has subsequently borne. Rümker found it at Paramatta on the 2nd of June, 1822, nine days after it passed its perihelion; and it has been observed at every return which has taken place since that time.

Yours faithfully,

W. T. Lynn.

Blackheath, 1888, May 17.

[The return of Encke's Comet this year will be under similar conditions to those of 1855, and the comet will thus not be observable in these latitudes either before or after perihelion. We have been asked also about Faye's and De Vico's comets. Faye's comet has been for some time in Dr. Axel Möller's hands, and an ephemeris will no doubt appear shortly. De Vico's comet may be regarded as lost, not having been certainly recognized since 1844.

May 28.—An ephemeris of Encke's Comet, by Drs. Backlund and Seraphinoff, was received this morning. The comet is distant from the Sun on June 1 only 26 min. in R.A. and 3½° in N.P.D.; on July 1, 73 min. in R.A. and 4° in N.P.D.; on August 1, 3^h in R.A. and 33° in N.P.D.; so that it will hardly be observed before the middle of July, when it is going rapidly south, crossing the equator on July 20.—Eds.]

Appulse of Jupiter to β Scorpii.

Gentlemen,—

The second appulse of Jupiter to this star this year was observed under such favourable skies as to render it a matter of some interest and curiosity. On the last occasion, in 1876, the conjunction was probably closer and may have been an actual occultation; but cloudy weather seems to have prevailed in England. A diagram of the phenomenon as observed in Ireland will be found in the 'Ast. Register' of that period.

On May 18 the planet, four satellites, and star were easily visible in the same field with lowest micrometer eyepiece on a 3¼-in. The star was sufficiently on a parallel with the planet and satellites to make it resemble a fifth satellite a little brighter than the rest; 15′ distant from Jupiter this night.

May 19.—Less haze than last night. The four satellites on the same side of Jupiter as the star. Very rough and hasty micrometer measurements with the lowest eyepiece at 10.45 gave

Distance from star to Jupiter's nearest limb about .. 7′ 20″.
Distance from star to IV. Sat. (the nearest sat.) about 1′ 38″.

May 20.—It was evident to-night that there would be no actual occultation. The brightness of the companion of β Scorpii very similar to that of the planet's satellites. At $10^h\ 30^m$ approximate measurement gave for distance of star from planet's nearest limb 1′ 43″.

May 21.—Planet had receded a considerable distance from the star.

Faithfully yours,
S. J. Johnson.

Melplash Vicarage, Dorset,
1888, May 22.

OBSERVATORIES.

Wolsingham Observatory.—In Circular No. 20 Mr. Espin states:—"A star, Mag. 8·1, not in the DM., was observed here on the nights of May 8 and 9, R.A. $20^h\ 41^m\ 7^s$, Decl. +44° 24′ (1855). The colour of the new star is red; the spectrum is not continuous."

THE LICK OBSERVATORY.—The first volume of the publications of this Observatory, containing 314 pp. quarto, has recently appeared, and gives an interesting account of the preliminary operations and the establishment of the buildings and instruments. In an introductory note Capt. R. S. Floyd, the president of the trustees, says:—"The completion of the task entrusted to the Lick Trustees by the provisions of Mr. LICK's Deed of Trust is now apparently near at hand. This task was to construct and erect 'a powerful telescope, superior to and more powerful than any telescope ever yet made, with all the machinery appertaining thereto and appropriately connected therewith * * * * and also a suitable observatory.' The present Board of Trustees was appointed September 2, 1876, and has had this object in view continuously for the past ten years. In the course of this time members of the Board have visited many of the leading observatories of this country and of Europe; the principal astronomers of the world have been advised with personally and by correspondence; thousands of letters have been written to them, to architects, contractors, builders, and instrument makers; and every detail of the construction and equipment of a vast astronomical establishment on the summit of a mountain 4000 feet in height and 26 miles distant from the nearest town has been personally superintended. It is impossible to convey in a few words any adequate idea of the multiplicity of separate interests which have been considered—from those of the practical astronomer to those of the day-labourer—nor of the distressing legal complications which have arisen and are now happily settled; but it will be found to be interesting by those who will read this and subsequent volumes of the Observatory Publications, to remember the very exceptional nature of the duties confided to us. We have been obliged to make the summit of Mount Hamilton accessible by a road 26 miles long, to remove more than 70,000 tons of material in order to get a level platform large enough for the instruments to stand upon, to arrange a good and sufficient water supply on the top of a barren mountain, to maintain and provide for workmen of all classes, and to carry out in the best and most economical manner the real object of our trust, which was to present to the world an astronomical observatory of the highest class which should be permanently useful to science."

The contents of the volume are roughly as follows:—Mr. Lick's deeds of Trust; Prof. Newcomb's report on glass for objectives; Report of Mr. Burnham's work at Mt. Hamilton in testing the climate for double-star work in 1879 and again in 1881; Descriptions of the buildings and instruments; an account of the engineering and building at Mt. Hamilton in the years 1880–1885; Observations of the transits of Mercury in 1881 and of Venus in 1882; Geological reports; Meteorological observations, 1880–1885; Reduction tables for the Lick Observatory.

This volume in fact contains little that has not already appeared in other publications; but it is chiefly important as being the first of what we hope will be a long and valuable series. We congratulate Prof. Holden on having now got his hand firmly on the plough which it has taken so much anxiety and labour to construct. An article on the observatory was contributed by him to the 'Sidereal Messenger' for February, from which we extract the following paragraphs:—

"It was found that the nights of summer and autumn, say April to October or November, were excellent both as to clearness of vision and as to steadiness. The daylight hours are less satisfactory. Mr. Keeler has lately shown that the vision in winter time is not specially better than at lower elevations."

"The secret of the steady seeing at Mt. Hamilton lies in the coast fogs. These roll in from the sea every afternoon in the summer, rising 1500 to 2000 feet. They cover the hot valley, and keep the radiation from it shut in. There are no fogs in the daytime and few in the winter."

"At the south end of the observatory is the 75-foot dome (for the 36-in. refractor). At the north end is the 25-foot dome (for the 12-in. refractor). They are connected by a hall 191 feet in length. On the west is a series of study and work-rooms. For the next twenty years there will be space in these rooms and in the hall for all the work of the Observatory. When it is necessary a second row of rooms can be built on the east side of the hall. I do not see any possible expansion of the Observatory which cannot be provided for by such additional rooms, and by separate detached observing-rooms in the immediate vicinity."

The complete instrumental equipment of the Observatory is as follows:—Equatoreals of 36, 12, and 6½ inches aperture; 4-in. Comet-seeker; Photoheliograph; 6-in. Meridian Circle; Declinograph; 4-in. Transit and Zenith telescope combined; 2-in. Universal instrument; 3 chronographs; 5 independent clocks, besides controlled clocks and chronometers; minor astronomical and a good set of meteorological instruments.

PULKOWA OBSERVATORY.—Vol. xii. of the Observations promised by Prof. Struve in his last report (see 'Observatory,' No. 133, p. 135) has now been published, and contains the catalogue for 1865·0 of the principal stars to the 4th magnitude as far as −15° declination, deduced from observations in the years 1861–1872. The formation of a catalogue of these stars for the epoch 1845·0 was the first piece of work completed by this Observatory; and the present work is thus enhanced in value by being an almost exact repetition, allowing for modern improvements, of that undertaken 20 years before. Thus Volumes xi. and xii. deal with the right ascensions obtained by the transit-instrument of 6-in. aperture (reversible in the pivots and also end for end), and correspond to Vols. i., ii., and part of iii. The declinations are dealt with in Vols. xiii. and xiv., which correspond to the earlier Vols. iv. and v. But the catalogue of final results for both elements is given in the present volume xii., whereas the R.A. and declination catalogues were formerly somewhat inconveniently separated in Vols. i. and v.

In turning to the earlier volumes for some information as to the possible alteration of conditions, we find that the first experience of M. Peters with the transit was a somewhat distressing one. After eight months' hard work with it, he found that enormous fluctuations of azimuth with temperature, amounting to $1''\cdot75$ per degree, so far interfered with the accuracy of his results as to make it unadvisable to use them for the Catalogue. The fluctuations appeared to be due to the fact that the foundations of the piers had not dried thoroughly even after two or three years. Towards the middle of 1841, however, this effect disappeared, and the observations were recommenced by M. Schweizer. The use of meridian marks in conjunction with stars to determine the azimuth, introduced to meet this difficulty, has been continued at Pulkowa to the present time. There are two somewhat important differences between the Catalogues of 1845 and 1865 as regards the Right Ascensions. The observations for the former were made by four different observers—Schweizer, Fuss, Lindhagen, and Wagner; those for that of 1865 by Wagner alone; and, secondly, many, though not all, of the observations for the second catalogue were made by the chronographic method. We may perhaps add that the second catalogue has the benefit of the considerable experience gained during the construction of the first, notably by the addition of an investigation of the personality depending upon the rate and direction of motion by means of transits of an artificial star, and a careful comparison between the chronographic and the eye-and-ear method of observing. The whole of the discussion of the results which forms the introduction to the present volume is, in fact, a development rather than a repetition of those in Vols. i. and iii.

The number of stars in this Catalogue is 381, 7 of those in the 1845 Catalogue having been excluded as insufficiently observed, and 14 added.

PUBLICATIONS.

Cincinnati Zone Catalogue.—Mr. Porter has published, as the result of two and a half years work with the 3-in. transit of the Cincinnati observatory, a catalogue of 4050 stars between declinations $-18^\circ\ 50'$ and $-22^\circ\ 20'$, most of the stars down to magnitude 8·5, as well as many fainter ones, having been observed. The average number of observations is almost exactly three. The probable errors of a single observation are $\pm 0^s\cdot123$ in R.A. and $\pm 1''\cdot84$, which are creditable considering the small size of the instrument, and the low power (30) necessarily employed to obtain a large field. A comparison with the Argentine General Catalogue seems to show that there is little or no systematic error in R.A., the differences changing sign from hour to hour, and being comparable with their probable errors. Another comparison with Yarnall's catalogue is given; but for the first 15 hours of R.A. there is practically no material available.

The proper motions of 75 stars have been determined from comparisons of their observed places with older catalogues, and are given in an Appendix.

REPORT OF THE METEOROLOGICAL COUNCIL OF THE ROYAL SOCIETY FOR THE YEAR ENDING 31ST OF MARCH, 1887.—This report contains the usual information and statistics concerning the work of the Meteorological Office for the year mentioned. Part I. gives an account of the progress of Ocean Meteorology. The great work of investigating the weather over the North Atlantic Ocean for the 13 months beginning August 1, 1882, has continued to employ nearly the whole Marine Branch of the Office; and steady progress has been made with it. The reduced drawings, from which the lithographed charts are produced by the help of photography, have to be prepared for the months of June, July, and August, 1883; but beyond this there remains but little to be done. The reduced daily charts are in pairs, one representing the atmospheric pressure and wind, and the other the air and sea temperatures, weather, rain, &c. A portion of this work has already appeared; and the Meteorolegical Office is to be congratulated on the approaching completion of so valuable and important a contribution to meteorological science. Part II. relates to Weather Telegraphy. It would seem that under present conditions it is not possible to improve much the forecast of gales, the warnings which have been justified in each year showing only a slightly increased percentage during the last ten years, pointing to the importance of increasing, if possible, either the number of reporting-stations or the daily number of weather telegrams sent from each to the central office. Part III. gives information on the Land Meteorology of the British Isles. From an attached report it would appear that the existing system of Ben-Nevis telegrams affords the office no assistance in the preparation of storm-warnings. Neither does it seem that the transatlantic messages are at present of any great value for the purpose of forecasting, indeed sometimes appear to be misleading. At page 22 of the report an interesting table of the distribution of gales in the British Isles is given, referring to the 15 years 1871–1885. But what will, perhaps, more interest astronomers is the list of stations at which continuous records of sunshine are kept, of which in the British Isles there are 35, not counting some other stations which do not report to the Meteorological Office. W. E.

ANNUARIO METEOROLOGICO ITALIANO 1888.—This little annual, published under the auspices of the Italian Meteorological Society, and of which this is the third issue, has for its object the giving of useful information to meteorologists, much in the same way as is done for astronomers in the various excellent little annuals which have been for many years published abroad, but of which in England we have no exact counterpart. The work, which extends to above 300 pages, is divided into three parts. The first contains such astronomical and other related information as may be useful to meteorologists; the second gives lists of meteorological stations, tables for reduction of meteorological observations, and agricultural notes for the different months; the third, and larger part, consists of geographical and topographical information, tables of units of measure and of astronomical and physical units, with numerous papers on subjects either meteorological or having close relations with meteorological science.

NOTES.

THE TOTAL SOLAR ECLIPSE OF 1887, AUGUST 19.—The meteorological observations made by Messrs. Winslow Upton and A. L. Rotch, two American observers, at Iwanowo, in Russia, are given in an interesting paper in the 'American Meteorological Journal.' The results are summarized in the concluding paragraph as follows:—

"The temperature and humidity fluctuated, as is usual in eclipses, the former showing a fall and the minimum occurring a few minutes after the total phase, and the latter showing a rise, which in this eclipse was to the saturation-point. The minimum temperatures recorded by a thermometer in the shade, and by black or white bulbs *in vacuo* freely exposed, differed but a few tenths of a degree from each other, and were also a few tenths lower than the air temperature at 4.13 A.M., twenty-three minutes before sunrise. Minute fluctuations of barometric pressure, and of the wind's direction and velocity, were observed, which cannot with certainty be attributed to the eclipse, but which in the case of the wind can be so ascribed with some probability. The cloud tints show the appearance of clouds, some of which were within and others without the region of the eclipse. The most marked features observed were the long-continued deposit of dew and the great brightness of the landscape during the total phase."

It is to be further remarked as regards the barometric pressure, that Mr. Upton hopes to deduce the small mean fluctuation from the results obtained at a large number of stations with which he was in communication.

It was incidentally found during these observations that the temperature coefficient of a Hottinger aneroid which had been used at Caroline Island in the eclipse of 1883 was given by the maker with the wrong sign; so that an apparently sensible barometric fluctuation observed in that eclipse was really only a secondary temperature effect.

COMET *a* 1888 (Sawerthal) has apparently shown a remarkable change of brilliancy. On Wednesday night, May 15, I found it at 71° Z.D., but a slight haze came up and stopped further observation. It appeared very faint; but this may have been due to the approaching haze. The two following nights were cloudy. On May 18 midnight I found the comet in what appeared to be a very thick sky at Z.D. 79°; but the stars in the neighbourhood seemed to show that there was really a clear patch of sky here. The nucleus appeared to be about 8th magnitude, and not much brighter than the 10′ or so of tail visible. On Wednesday, May 23, which was the next opportunity I had for looking for the comet, and when I fully expected that the moonlight would prevent observation, I found the appearance so much changed that at first I thought it must be another comet. The nucleus was quite bright, I should say 5th magnitude at least, and no tail was visible: there was a wisp of nebulosity extending from the nucleus on either side. A careful verification of the place showed that it was still the same comet; and next morning a postcard was received from Prof. Krueger saying that Prof. L. Schwarz of Dorpat had telegraphed an increase of brightness of between 2 and 3 magnitudes between Sunday and Tuesday. Preparations were made for observing the comet with the spectroscope on Thursday, but it was cloudy both Thursday and Friday night. On Saturday, May 26, I looked carefully for the comet, of course in strong moonlight, but could not find it. It will be interesting to see what has happened to the comet when the Moon is more favourably situated.

The following ephemeris is taken from the 'Astron. Nach.' No. 2838:—

Ephemeris for Berlin Midnight.

	R.A.			Decl.			
1888.	h	m	s	°	′	Log r.	Log Δ.
June 3..	0	30	18	+40	39·6	0·2052	0·2826
7..		36	12	41	52·8	0·2205	0·2899
11..		41	39	43	2·2	0·2352	0·2965
15..		46	38	44	8·3	0·2493	0·3025
19..		51	9	45	11·4	0·2629	0·3080
23..		55	11	46	11·5	0·2760	0·3129
27..	0	58	42	47	8·9	0·2887	0·3173
July 1..	1	1	42	+48	3·7	0·3009	0·3212

H. H. T.

Precision Clocks.—The Seth Thomas Company, whose cheap clocks have now become so popular, have in process of construction a high-class regulator which they propose to call a "Precision Clock." From the pamphlet before us we gather that they have not entered lightly on this undertaking. The clock is the result of their own experiments combined with the experiences of horologists in all parts of the world. The frame is massive and very heavy, being of cast iron and resting on three levelling-screws. Into this is fixed a solid triangular casting for supporting the pendulum suspension-springs. The cast-iron case is intended, first, as a rigid support for the pendulum, so that, tested by delicate telescopic measuring-apparatus, the pendulum swings practically in a true plane; secondly, as a magnetic shield against all ordinary magnetic disturbances, leaving the pendulum free to swing in a uniform magnetic field; thirdly, from the rigidity of the case, and the fact of all its surfaces and joints being true, it can be made sensibly air-tight. The weights hang in brass tubes outside the clock-frame proper. Mercurial compensation is adopted for the pendulum. Electric-contact apparatus is fixed if required. This apparatus and the whole clock-movement is exposed to view through the plate-glass sides and front. Two forms of escapement can be fitted at the option of the purchaser—one a dead beat on Graham's principle, and made of aluminium (first suggested, we believe, by Lord Grimthorpe for the sake of lightness); the other is the single three-legged gravity-escapement, which is a modification of the Denison gravity-escapement applied to the Westminster clock. T. L.

THE SOLAR ROTATION PERIOD FROM OBSERVATIONS OF FACULÆ.—Dr. J. Wilsing, of the Potsdam Observatory, has recently made a large number of measures of the positions of faculæ on photographs of the Sun taken at Potsdam from 1884, March 1 to August 31, with a view to determining the period of rotation of the solar layer to which they belong. The faculæ can in general only be traced near the limb, and cannot be identified from day to day as they travel across the visible hemisphere. But if the longitude and latitude be determined at one limb, the facula can be looked for at the time when it should reappear at the other after the lapse of an odd number of semi-rotations, or at the same limb after the lapse of a number of whole rotations. A comparison of the positions in longitude at the different returns gives data for determining the rotation period with considerable accuracy, allowing always for the difficulty of satisfactorily estimating the position of such ill-defined objects as faculæ. Dr. Wilsing obtains the interesting result that the layer containing faculæ apparently rotates as a solid body; that is to say, the faculæ in high latitudes do not appear to lag behind those near the equator, as Carrington and others have found in the case of spots. He finds the angular velocity in degrees per day to be 14·2718 from spots in the northern and 14·2686 from spots in the southern hemisphere; the mean 14·2698 of these very accordant results corresponding to a period of $25^{d}\cdot2281$ or $25^{d}\ 5^{h}\cdot47$. The period of magnetic disturbances was found by Hornstein to be $24^{d}\cdot55$, but by Brown $24^{d}\cdot18$ and $24^{d}\cdot23$ by separate investigations, which latter results agree well with the faculæ period. Perhaps the least satisfactory part of Dr. Wilsing's discussion is that the faculæ are far from being identified at every return; and doubts may be raised as to whether observations of two faculæ in the same part of the Sun's surface which are separated by an interval of say two rotations and a half, while nothing has been noticed in this neighbourhood at any of the intermediate 4 occasions of possible observation, can really relate to the same disturbance.

Minor Planets.—No. 277 was discovered by M. Charlois at Nice on May 3, No. 278 by M. Borrelly at Marseilles on May 12, and No. 279 by Herr Palisa at Vienna on May 16. No. 273 has been named Atropos and 274 Philagoria.

We notice in a recent number of 'Sirius' (vol. xvi. p. 84) that a company has been formed, with a capital of £10,000, for the somewhat novel purpose of encouraging and spreading the pleasures of learning about Nature (die Verbreitung der Freude an der Natur erkenntniss). The headquarters of the company is to be Berlin; its name is to be the "Gesellschaft Urania;" Dr. Förster, of the Berlin Observatory, is to be its first president, supported by a committee of four; and the qualification for membership is friendliness towards Nature. The plan of operations is to some extent educational: opportunities will be given freely to those to whom they have hitherto been denied for seeing the sights of the heavens; and the brilliant idea has been evolved from some truly great intelligence of defying all climatic difficulties by having a reserve show of artificial eclipses, transits of Venus, comets, auroræ, and the like, which are to be as natural as life and twice (or more) as large. But this is merely the educational side of the project. From another point of view it is to be a society of festive priests of nature, who are to welcome each returning comet with befitting rites; to take care that not only a Transit of Venus but its Jubilee be duly observed; and to celebrate with high revelry the centenaries of the oldest recorded eclipse. Good old Gesellschaft!

The Royal Society held their annual conversazione on May 9. The arrangements and exhibits were extremely successful, and a thoroughly enjoyable evening was spent by a numerous company. Mr. Boys showed some very pretty soap-bubble experiments, and Mr. Wimshurst's electrical machine gave some splendid sparks. Captain Abney and General Festing's colour experiments and Prof. Rücker's optical demonstration of electrical stress also attracted interested audiences throughout the evening.

A Committee on Solar Eclipses has been appointed by the Royal Astronomical Society. According to one of the resolutions passed by them, it is felt to be desirable that the objects of this Committee—viz. (1) to bring into communication with each other gentlemen wishing to observe total solar eclipses, and (2) to become a centre of information for intending observers—should be made known as widely as possible.

Herr F. K. Ginzel would be glad of any as-yet-unpublished observations of Comet *e* 1887 (Olbers-Brooks). Address to the Berlin Observatory (Lindenstrasse 91).

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 138. JULY. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, June 8, 1888.

W. H. M. Christie, M.A., F.R.S. (Astronomer Royal), *President*, in the Chair.

Secretaries: E. B. Knobel and Col. Tupman.

The Minutes of the previous Meeting were read and confirmed.

Col. Tupman. We have received 48 presents since the last Meeting. There is scarcely anything calling for special mention except the report of Admiral Mouchez on the work done at the Paris Observatory. It contains a reproduction of the truly astonishing photographs obtained by the Bros. Henry of the Nebulæ in the Pleiades.

Votes of thanks were accorded the donors.

One or two Fellows were then admitted by proxy.

Mr. Chambers. Mr. President, may I ask as a matter of order whether there is any precedent for the admission of Fellows by proxy?

The President. I may refer you to the Bye-Laws, Mr. Chambers. You will find there is a Bye-Law providing for that.

Mr. Knobel read the Bye-law relating to the admission of Fellows by proxy.

Mr. Chambers. I am quite satisfied with the explanation, but I think the process ridiculous all the same.

The President. I think my function is to carry out the Bye-laws and not to criticise them.

Mr. Knobel read a "Note on the Glasgow Star-Catalogue, by Professor Grant." Professor Grant said that it was now five years since the Star-Catalogue was first published, and it was gratifying to him to find that it had been largely used by astronomers in connection with extra-meridional observations of minor planets and comets. This circumstance had encouraged him to endeavour to increase its usefulness still further by a re-observation of the stars in the Catalogue the places of which, when compared with Bessel's places, exhibited larger discordances than could be accounted for by the ordinary errors of observations. The observations would be ready for printing about the close of next year, and would be in the hands of astronomers in the following summer.

Mr. Knobel read a communication from Dr. C. H. F. Peters of Hamilton College, giving some observations of Sappho, which he observed had been necessarily limited in number by the state of the weather.

Mr. Bryant. I am glad to hear that Dr. Peters has been able to make even these few observations. It seems, as Father Perry wrote to me a few days ago, that it is not only eclipse observers who suffer from the bad weather. He had the intention of making a good series of observations during the last opposition of this planet, but unfortunately was prevented, as Dr. Peters appears to have been, by bad weather; and he could only secure one. The work I am engaged upon was originally contemplated in 1884 for the purpose of determining the solar parallax. As Fellows are doubtless well aware, observations were undertaken by Dr. Gill in 1882 by the heliometer method for the determination of the Sun's distance, but unfortunately the errors of the planet, both in right ascension and in declination, were so great that his choice of comparison-stars was by no means the best that could have been wished for. The errors of the planet in right ascension amounted, roughly speaking, to 400 seconds of arc, and in declination to about 100 seconds; these were far larger than were expected, but the calculation of the perturbations does not seem to have been carried out with great accuracy, that is according to a letter I have received from Prof. Tietjen of Berlin. As I have previously remarked, I contemplated this work first in 1884; but it was only at very disconnected intervals that I could pay any attention to it. I was not quite certain that Sappho would be the most suitable object. Victoria offered certain advantages; Ariadne was also suitable. I communicated with Dr. Gill on this point. Although I had previously desired to take Sappho, I thought it best to communicate with Dr. Gill, and he wrote suggesting Victoria or Sappho; and in 1885 I began systematically the work, which, as originally undertaken, was merely meant to give an accurate representation of its orbit and prepare an ephemeris for September and October next year, at which time a very favourable occasion will present itself for determining the solar parallax.

Since 1885 I have carried on the work with very little interruption. It has been one of very great labour. I have had to take into consideration the perturbation of Venus, the Earth, Mars, Jupiter, and Saturn from 1863 to 1890. The perturbations by Venus were originally calculated at intervals of forty days, as also the perturbations of the other planets. The irregularity of the differences, however, in the case of Venus were somewhat objectionable; and I communicated with Professor Oppolzer on the point, and, acting on his advice, reduced the interval for Venus to twenty days when the differences became less irregular. The work is very laborious; even the weeding-out of the perturbations, which is absolutely requisite, has required about 2,000,000 of figures; and I am single-handed in the matter. Unfortunately, we cannot well apply the usual planetary theory to this body, because the eccentricity is so great and also the inclination of its orbit to the plane of the ecliptic, so that if we adopt the ordinary methods applied to the major planets, we shall have to carry the terms to a very much higher order than usual. Although this work was originally undertaken for the calculation of an accurate ephemeris only for the year 1889, as I progressed with the matter I thought it was a great waste of labour, if that were the sole object in view, to undertake so much work, and I have thought it advisable therefore, if possible, to obtain a correction to Jupiter's mass, which I think is possible considering that most of the disturbing causes will be weeded out. It is true that at Washington the correction of Jupiter's mass is being obtained from the perturbations of Polyhymnia; but I have no information of the mode of procedure beyond the mere statement of that fact. With regard to Sappho, the angle of eccentricity is between 11° and 12°, giving an eccentricity of one-fifth, and on account of this eccentricity the planet approaches fairly close to the planet Jupiter and undergoes considerable perturbation; so it is possible, I think, to obtain the correction required. But there is one point to which I think sufficient attention has not been directed and which we ought to consider. If we seek to determine the secular effect of one planet upon another, the expressions which give this secular effect involve in their denominators the difference of the major axes of the orbits. As a consequence, if we find two major axes which are very nearly equal this secular defect will be enormous. Such a case does not occur among major planets, but their masses are very much larger than the masses of the Asteroids, so that they have to be taken into account. But in investigating this matter with regard to Sappho, we find Melpomene, No. 18, which has a major axis different in about the fourth decimal, and in the square and cube the coefficient becomes enormous; it amounts to millions and millions. Of course that has to be multiplied by the mass of the planet; but from some preliminary computations I was making at the beginning of this week it seems that if we assume a radius of about 40 miles to Melpomene the secular effect upon Sappho would not be enormous though still great, and in the course of a quarter of a century, over which the calculations extend, would amount, I think, to about 50 seconds of arc with regard to the node, but with regard to the longitude of the epoch they would be very much greater. On this point, however, I cannot yet speak with certainty. My calculations are not yet completed, and I have only made certain rough investigations on this point at present. I have already sent to Dr. Gill an approximate ephemeris for next year, so that he will be able to select the comparison stars; but I hope before the beginning of next year to be able to place before him an accurate representation of the planet's path in the autumn of next year, by which, if necessary, he may be able to substitute for any unsuitable comparison stars which he has already chosen. I regret I had not the opportunity of putting together a few more facts in connection with this work, and I must ask you to accept what I have already given you.

A vote of thanks to the Author of the paper was passed.

Dr. Copeland read a paper on "Comet Sawerthal"; which he said had been put together by his zealous assistant, Dr. Becker, to whom observers of comets are indebted for so many orbits. The paper had reference to the jets which seemed to issue from the nucleus of the comet, and to estimations of the brightness of the nucleus which, when seen in an illuminated field, was about equal to a ninth-magnitude star.

Dr. Copeland gave a sketch of the comet on the blackboard. Mr. Knobel added another sketch made by Mr. Biggs.

With reference to another paper on this Comet, communicated by the Astronomer Royal,

Mr. Turner said the Greenwich observations in the paper are chiefly made with reference to position, but in the notes references are made to the extraordinary change in brightness which the comet appears to have experienced. It certainly was a very striking change in brightness, and I am somewhat puzzled by the magnitudes which Dr. Copeland has attributed to the comet on May 21st and May 23rd. I am aware there is very great difficulty in estimating the relative brightness of a diffused object like a comet and a minute object like a star; but the change in the brightness of the comet was so striking that I should have thought units of magnitude instead of tenths were concerned. I looked for it on the 23rd May (Wednesday), because it had not been seen for the last few nights. I had seen it on the preceding Friday night rather low down and in what seemed to be rather a thick sky. Apparently there was just a clear patch near the comet. When I found it on May 23rd it was so different in appearance that I thought I had set the circle wrong and had discovered a new comet. But apparently it was the same comet on looking at the circles. I should have put it down as something like a 4th or 5th magnitude star instead of being a 9th. I may say it was a distinct and bright object in a field of bright moonlight; the Moon was shining brilliantly. Mr. Thackeray makes a note, on that night, that the comet has a well-defined nucleus, and he made a remark the next day—though perhaps not a perfectly independent one—that it was like a 4th or 5th magnitude star, showing brightly in strong moonlight; and he noted also that the colour had quite changed, viz. from a pale white colour to a brilliant yellow. The shape is very well shown by both the drawings on the blackboard. The filling up to which Mr. Knobel referred depends upon whether you included that faint portion of the line; and the word "winged" which both observers have used is an extremely good method of describing the appearance. We received a postcard on Thursday morning to say that the increase of brightness had been noticed in Russia and had been noted as an increase of two or three magnitudes, and we thought that a very fair representation of what had happened. But I am quite sensible that there must be a very great difference of opinion in the estimation of brightness. I last saw the comet two nights ago and I noticed that it had got back again to the 9th magnitude as before.

The President. It seems to have been a remarkable fact, this sudden increase of brightness, and we must regret that the spectroscope was not applied to the comet. As regards the discrepancy in the estimated magnitude between Dr. Copeland's observations and the Greenwich observations, possibly it may be accounted for to a certain extent by the circumstance that the observation at Dunecht was made in an illuminated field.

Dr. Copeland. Exactly.

The President. And that would have a great effect upon an object like a comet no doubt. I may mention also, as tending to explain the discordance, that the Greenwich observers are naturally used to estimating the magnitude of stars as seen in an illuminated field. Therefore when they see the object in a dark field their tendency would be to overestimate the brightness; I do not say they would necessarily be misled, but there would be a tendency in that direction if anything.

Dr. Copeland. I may say the estimated magnitudes at Dunecht are simply of the nucleus, when every other part would be only feebly seen even if no illumination of the field had been used. As to the amount of twilight, a very short-sighted person could read the preface of the 'Nautical Almanac' on the 21st June without any light at midnight, so of course there is a very strong illumination of the sky, even now, which combined with the red illumination of the field of view would entirely restrict the estimated light to that of the nucleus. To compare the two drawings, the Dunecht sketch should be about one-third the extent of the other one.

The President. I must say there seems to me a very satisfactory agreement between those two sketches, considering that they refer to a nebulous object. It is satisfactory that we have got such a good representation of this comet.

Dr. Copeland then read "A note on the visible spectrum of the Great Nebula in Orion." Dr. Copeland observed that when examined with a sufficiently powerful spectroscope the light of the spectrum was divided into three well-known lines corresponding to gaseous nebulæ. There was also usually a fourth line seen, which was generally admitted to coincide with the third great line of the hydrogen spectrum. Towards the end of 1886, however, when the nebula was first studied at Dunecht with an exceptionally suitable spectrum, a long stretch of continuous light was detected embracing the three brighter lines and terminating somewhat abruptly towards the red. Separated from this continuous spectrum was a well-defined bright line identical in position with D_3. The appearance of this line in the spectrum of a nebula was of great interest as affording another connecting link between gaseous nebulæ, the Sun, and stars with bright-line spectra, especially with that remarkable class of stars of which the first specimens were detected ay Wolf and Rayet at Paris. On January 23rd, 1887, it was believed that traces of an exceedingly faint line could be made out further towards the violet with a wave-length of about 448; but if a line really existed in that part of the spectrum there would probably be no difficulty in photographing it. A table appended gave the wave-lengths of different sets of measures, and the wave-lengths determined in no case differed more than one unit in the fourth place of decimals from the best results received.

The President. What was the form of spectroscope used?

Dr. Copeland said it was a very fine spectroscope which had been made for Lord Crawford by Messrs. Thomas Cooke and Sons of York. It was as simple as possible. The collimator was the same proportion as the large telescope, that is 12 focus to 1 aperture; the big telescope is 15 in. and 15 ft. and the collimator is 2 in. and 2 ft. It was a most beautiful and satisfactory instrument, the only drawback being its great weight, 160 lbs., which made it awkward to mount. So that when the micrometer was in position with the telescope the spectroscope was not and *vice versâ*. There was quite a variety of prisms. There was a prism of something like 20 degrees, one of something like 35; another, that which was principally used, was exactly 60; and then there was a magnificent compound prism made by Grubb, a wonderful thing. This prism showed almost every line of Angström's spectrum. Most of the measures of spectra of nebulæ had been made with the simple prism of 60°.

Mr. Common. I should like to make a few remarks upon the great advance that has been made in photography lately. Everyone knows the Pleiades and the Nebula which I think Tempel discovered some years ago. Now the Pleiades have again been photographed by the Brothers Henry, and the result of their latest work is given in the last report of the Paris Observatory, where a beautiful engraving of the plates they have succeeded in taking is given. My chief reason for wishing to bring this before the Society on this last night before the long vacation is to draw the attention of astronomers to it as illustrating in about the best possible way just what photography can do. I think if, instead of discoursing on the advantages of photography, one just took this chart and considered it in all its bearings with relation to what has been done by observers on this group, what we know now to be there, and what we can see with the ordinary method of eye-observation, he would not need any further proof of everything that is claimed for the photographic method of observing. The Nebula as we first knew it was represented by a patch near Merope, the exact position of which few observers agreed about; indeed the very existence of the nebula was questioned by two observers in America a few years ago.

In 1880, using the 3-feet telescope, I gave two or three hours to this object and I found that in addition to the Merope Nebula there was, between Alcyone and two stars near, another nebula; and that nearly on Merope there was also another. It was the publication of this sketch in 1880 that brought the remarks from Professor Tempel that are published in Vol. xl. of the 'Monthly Notices,' where Professor Tempel gives a picture of the Nebula as he saw it, and as it was generally supposed to exist. Mr. Roberts has taken some photographs of the Pleiades, which you may have seen in the library of the Society; but owing to the fact that the driving of the clock was not so accurate as could be wished and that halation comes in in an objectionable manner, it is exceedingly difficult to trace all nebulæ which we now see, from the engraving of the photographs of the Brothers Henry, are there; but all or almost all is shown—indeed, in some parts, there are indications of more than is shown on the engraving.

Working in 1880 under favourable circumstances with a 3-foot aperture, I could not see certainly more than I then indicated in a short paper in the 'Monthly Notices,' in Vol. xl. p. 376. But here, with much smaller instruments, photography has shown very much more not only of the nebulous light but of the actual shape of the various nebulæ which it is almost impossible to define by eye-observation. It is not only in the nebulæ that photography is so successful, but in showing the position of faint stars; for, out of curiosity, I took the place of various small stars near Alcyone—stars just visible with a 3-foot aperture—and, of course, as soon as I got the engraving referred to, I immediately compared my sketches with it and found that every star I had seen, except one, was there, and, of course, in their proper places. (Mr. Common showed on the blackboard the position of the stars as observed and as photographed.) Of course, as Sir J. Herschel remarked, it is always easier to see a thing we know to be there than to discover it; but even now I think it will be a severe test for any telescope to show all these stars. As to the nebulæ we have now been able to find by photography, I think they never could have been discovered by the ordinary method, particularly that most wonderful streak of nebula that threads 8 or 9 stars. Altogether I think the work we have here in this engraving as representing the photographs of the Brothers Henry is of the very first importance, not only in itself, but in showing what can be done on other objects of which we now know only as much as we knew of the Pleiades six years ago.

Mr. Ranyard. There are one or two points about this wonderful photograph of the nebula in the Pleiades to which I should like to draw the attention of the meeting. In the nebulous line of light referred to by Mr. Common there are 8 stars, including the one just off the line. One can hardly doubt that they must have some physical connection with it. Curiously, the line is not quite straight, and there are two other linear masses of nebulæ nearly parallel with it. There is also a very remarkable parallelism in two other parts of the nebulæ. In fact, there are three sets of lines of nebulous matter more or less parallel with one another, but slightly curved. Another thing that is very striking about the photograph is the way in which the small stars are arranged in a sort of meshwork pattern. There are a number of small areas where there are no stars, though round them there are streams of stars, forming a meshwork of stars all over the plate. This is so striking when you examine the plate closely that one can hardly doubt that the fact has some physical meaning; possibly the light of the stars is cut out by opaque masses in the starless areas. There is something similar to this in the arrangement of other clusters.

Mr. Newall. Mr. Common and I had a discussion on this subject in this room in 1880. I made a drawing of what I saw of the nebulæ; Mr. Common made a drawing of his: he said he could not understand my drawing; I said I could not understand his. I do not know whether the Fellows understand either. But I have seen it several times since, and I am quite certain what I saw was there, but it differs from what Mr. Common saw a little time ago. It is a nebula very like an oval comet, Merope being in the focus of the nebulous matter surrounding it. I think I remarked at that time that I had observed throughout the whole constellation of the Pleiades a large amount of nebulous light. I have seen it over and over again, and it strikes me as being very visible indeed. I have not seen the patches of light which Mr. Common has on the blackboard. I have not had the opportunity of observing the Pleiades recently; but the next time I have the chance I shall certainly look at the constellation very carefully and compare it with the beautiful drawing in the 'Rapport Annuel,' of which Admiral Mouchez kindly sent me a copy. I hope that will be incorporated in our Transactions, because it is of very great assistance to those who are observing the constellation, and especially the nebulous matter.

In the 'Monthly Notices,' Vol. xl. p. 89, there is a communication from Mr. Maxwell Hall, December 1879, in which he says:—"Again, it has been seen through Mr. Newall's 25-inch refractor; and his verbal description of the nebula corresponds so closely to that I have always seen in Jamaica that my sketch made in 1877, after about a year's attention to the nebulæ, may prove valuable." I have made a drawing on Argelander's map of what I saw, and I gave Mr. Hall a copy of it when he visited me, and this compared exactly with his drawing referred to in the above communication, except that he has inserted more stars than there are in Argelander.

Mr. Knobel. We have present with us to-night Mr. Marth. I think it will be in the recollection of Fellows that Mr. Marth has called attention in the 'Monthly Notices' recently to lunar sketches which have been made, and the insufficiency of those sketches in regard to the times of observation. I hope Mr. Marth will be able to give us some remarks on this interesting point.

Mr. Marth. I did not intend to say one word publicly, but I did intend to say something privately. I have asked Mr. Knobel and Mr. Green and others, who are specially interested in lunar sketches and who are themselves draughtsmen, to be so good as to give me a list of the sketches which they have made. I have also been in correspondence with Mr. Elger in order to get information about any published sketches which may not have come under my notice. I am sorry to say the work which will fall to my share will be far smaller than I was prepared to undertake. It is 15 years ago that, in a paper referred to in the 'Monthly Notices' of March, I gave a list of 600 lunar points, with the co-longitudes of the Sun at sunrise and sunset. But in all the 15 years comparatively little has been done. Now I want at least to gather up what has been done, and those who have made sketches would oblige by giving a chronological list of them, and let us have them here if possible in the course of the next weeks.

It is certainly a reproach upon our knowledge of the Moon that we cannot say what phenomena will take place within any hour of observing; and yet there need not be the slightest difficulty if only those who make sketches will confine themselves to comparatively small portions of the Moon, and will look at their watches and put down the railway time when the shadows are drawn. But unfortunately, because it is far too simple to look at a watch at the right time, it is not done. The general list of sketches, which might be extended to thousands, will in consequence be confined to a few hundreds. However, it is possible that this limited list may stimulate those who are clever draughtsmen to make sketches and to time them properly, as they will find that their contributions help to advance our knowledge. In connection with this subject I may be allowed to say that if we could only get sketches of the nearly full Moon as seen with the naked eye or with the assistance of a small opera-glass, and then compare them with the sketches made with the help of telescopes, we should have a far better chance of judging by analogy what the sketches of Mars really teach; and the comparison of the naked-eye sketches of the Moon by different draughtsmen will afford us at least some sort of guide with regard to the accuracy of the sketches of Mars. I therefore call publicly on Mr. Knobel and Mr. Green and all those who are clever enough draughtsmen to do this simple thing. I myself am not a draughtsman, so I shall appreciate it all the more if they will be good enough to do it.

Mr. Knobel. Mr. President, it was, I think, in the year 1873, at Somerset House, that Mr. Marth first made this request to me; that I admit is a long time ago, but I have endeavoured to accept his challenge on a great many occasions, but the results that I have obtained have been so exceedingly unlike what I thought would be satisfactory to him that I tore them up. To draw the full Moon is an exceedingly difficult thing; it is vastly more difficult than drawing a few dark markings upon Mars; its being so brightly illuminated renders it excessively difficult to draw a picture which at all resembles what you see with the eye. In fact, I have tried a great many times to accept Mr. Marth's challenge, but I certainly am not in the least degree competent to furnish him a satisfactory answer; I wish I could. Of course, it would be very satisfactory if observers who delineate planets could offer a gauge of their powers by such a thing as Mr. Marth suggests, which would afford a test, so that we might value other results by the gauge which is furnished by their drawings of the full Moon; but the full Moon (as seen by the naked eye) seems to be almost beyond the powers of anyone to represent satisfactorily, either in black and white or in colour or in any way that we have at our disposal.

Mr. Common. Except photography.

Mr. Knobel. That is not delineation.

Mr. Green. If I might add a word to what Mr. Knobel has said, I would express my sense of the difficulty of lunar representation in this way. In the years that Mr. Warren de la Rue's photographs of the moon were first exhibited, I was making a series of drawings of the Moon and taking the utmost pains with them, but on seeing the photographs felt so thoroughly convinced that no drawings could ever compare with them that I gave it up. All that I had done was to draw portions of the surface; to represent the appearance of a full Moon as seen with even a small telescope would be beyond the power of any artist. Surely a good photograph ought to supply what Mr. Marth requires.

Mr. Marth. I beg your pardon, but photography does not supply what I want; I want that the Moon shall be drawn as a test-object for the sketches of Mars. I should like to compare the drawings of the Moon made by Mr. Knobel and Mr. Green and others. In that way we could get some sort of a notion as to how the same thing impresses the eyes of different artists.

The President. I think we have a very good illustration of how differently the same thing impresses the eyes of different artists when we look at the various drawings of the Orion nebulæ on the wall and compare them with the photograph. It is very curious to notice the fidelity of the artists to nature in the details, and at the same time the enormous difference in personality in the representation. I think that is an illustration of the difficulty to which Mr. Marth wishes to call attention here. Perhaps I may mention in connection with this that lately on a visit to Oxford I had the great pleasure of seeing a very beautiful drawing of the whole disc of the Moon by Mr. Nasmyth, which was made in the year 1849 and exhibited in the Great Exhibition, and which was a very beautiful production; but in that case it is rather a map of the Moon than a drawing of the full Moon, because the shadows are shown.

Mr. Knobel read a paper on "Physical Observations of Saturn in 1888," accompanied by notes on the appearances of the ring by *Mr. Elger*, which having been read, a vote of thanks to the author was passed.

The President. I have been asked by Professor Krüger to call the attention of the meeting to the fact that there is at present in this country only one subscriber to the Central Stelle for astronomical telegraphy, viz. Greenwich Observatory, while in France there are 5, in Spain 1, Portugal 2, Italy 5, Germany 12, Austria 4, Switzerland 3, Russia 12, Scandinavia 4, Belgium 2, Poland 2. In England, as I have said, there is only one subscriber, and Prof. Krüger appeals to English astronomers to show more interest in this international undertaking for spreading news of the discovery of comets and small planets. He points out that if telegrams about small planets and comets are desired, the contribution is at present £2 per annum (that is, for telegrams of the discoveries of comets and communication of ephemerides), and if the number of subscribers increases the contribution will be proportionately less.

Mr. Common. It might be desirable to give some publicity to this letter, Mr. President; there are many men in this country who do not know that by subscribing £2 a year they can have these telegrams (I, for one, did not), and if publicity were given to this thing you might get several additional subscribers.

The President. My impression is that full notice was given when this arrangement was made originally; but still I think the suggestion is a very good one, and it might be as well to call attention to it in the 'Monthly Notices.'

The following papers were announced:—

John Tebbutt. "Observations of Comet *a* 1888 made at Windsor, N. S. Wales."

Prof. C. H. F. Peters. "Observations of Sappho (80)."

Prof. R. Grant. "Note on the Glasgow Star Catalogue."

T. G. Elger. "Physical Observations of Saturn in 1888."

Sir G. B. Airy. "On a large Prime Number."

A. B. Biggs. "Observations of Comet *a* 1888 made at Launceston, Tasmania."

Radcliffe Observatory, Oxford. "Observations of Comet *a* 1888 (Sawerthal)."

Dr. L. Becker. "Note on Comet Sawerthal."

Royal Observatory, Greenwich. "Observations of Comet *a* 1888 (Sawerthal)."

Dr. R. Copeland. "Note on the Visible Spectrum of the Great Nebula in Orion."

The following gentlemen were proposed for election as Fellows of the Society:—

John Harvey Jones, Fairview Villa, Coburg Road, Montpelier, Bristol. (Proposed by W. F. Denning.)

William Henry Maw, C.E., 18 Addison Road, Kensington, W. (Proposed by Sir Howard Grubb.)

Albert Taylor, Hurstside, West Molesey, Surrey. (Proposed by J. Norman Lockyer.)

In accordance with the Bye-Laws the names of *M. Evans*, *S. Jefferson*, and *J. McCarthy* were ordered to be suspended as defaulters.

Meeting of the Royal Meteorological Society.

Wednesday, June 20, 1888.

DR. W. MARCET, F.R.S., *President*, in the Chair.

MR. F. DE B. COLLENETTE, L.R.C.P., M.R.C.S., Mr. J. Ewart, M.R.C.S., Mr. F. A. Velschow, and Mr. J. T. Wills, F.R.G.S., were elected Fellows of the Society.

The following papers were read:—

(1) "First Report of the Thunderstorm Committee."—This Report deals with the Photographs of Lightning Flashes, some 60 in number, which have been received by the Society. From the evidence now obtained it appears that Lightning assumes various typical forms, under conditions which are at present unknown. The Committee consider that the Lightning Flashes may be arranged under the following types:—1. Stream, 2. Sinuous, 3. Ramified, 4. Meandering, 5. Beaded or Chapletted, and 6. Ribbon Lightning. In one of the photographs there is a *dark* flash of the same character as the bright flashes; but the Committee defer offering any explanation of the same until they get further examples of dark flashes. As the Thunderstorm season is now coming on, the Committee propose to publish their Report at once, together with some reproductions of the photographs by the Autotype process, in order that observers may be prepared to notice the various forms of Lightning.

(2) "The Cold Period from September 1887 to May 1888," by Mr. C. Harding, F. R. Met. Soc.—The mean temperature for each of the nine months from September 1887 to May 1888 was below the average, whilst in the case of October there has been no corresponding month as cold during the last half-century, and only three colder Aprils. In London the mean temperature for the period was only 42°·4, and there has been no similarly low mean for the corresponding period since 1854–5, which will be remembered as the time of the Crimean War, and only three equally cold periods during the last 50 years. The temperature of the soil at Greenwich at 3 feet below the surface was below the average in each month from October to April; in October and April the temperature at this depth was the coldest on record, observations being available

for the last 42 years, and in November it was the coldest for 37 years.

(3) "Observations on Cloud Movements near the Equator, and on the General Character of the Weather in the 'Doldrums,'" by Hon. R. Abercromby, F. R. Met. Soc.—The author gives the results of observations made during four voyages across the Equator and the "Doldrums," with special reference to the motion of clouds at various levels. Two voyages were across the Indian Ocean during the season of the north-west monsoon, and two across the Atlantic in the months of July and December. The nature of the general circulation of the atmosphere near the "Doldrums" is discussed as regards the theory that the Trades, after meeting, rise and fall back on themselves; or, according to the suggestion of Maury, that the Trades interlace and cross the Equator; or, follow the analogy of Dr. Vettin's experiments on smoke. It is shown that the materials at present available are insufficient to form a definite conclusion, but details are given of the general character of the weather and of the squalls in the "Doldrums," with a view of showing what kind of observations are required to solve this important problem. The old idea of a deep Trade—with a high opposite current flowing overhead—is certainly erroneous; for there is always a regular vertical succession of the upper currents as we ascend.

Meteor Notes.

RECENT FIREBALLS.—On March 3, at $9^h\ 2^m$, a brilliant meteor was observed at many stations in the north of England, including Leeds, Blackburn, Wolverhampton, &c. The path was approximately recorded at a few places, but when compared together the descriptions are found to be inconsistent and the fireball's true path in the air cannot be satisfactorily assigned. From observations at Blackburn and Wolverhampton, which, however, show a good agreement as to the end point, it appears that this fine meteor at its extinction was 23 miles vertically over Dalton, in Lancashire.

March 4*, $13^h\ 11^m$. Fine meteor, $=$♃, seen by Mr. D. Booth at Leeds. The course was from $205° + 13°$ to $190° - 2\frac{1}{4}°$, along which a thin streak was left.

May 19, $11^h\ 7^m$. Small fireball seen by Mr. J. Reside at Carnlough, co. Antrim. "It appeared to emerge from δ Virginis, and after a rather slow flight disappeared above Regulus. Although the Moon was shining brightly at the time, it appeared almost as bright as Jupiter."

May 30, $10^h\ 58^m$. Very large meteor, rivalling ♀ at greatest brilliancy, seen by Mr. D. Booth at Leeds. Path from 251° −7½° to 258° −10°. Duration 2 seconds. It left a train of golden sparks.

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* This meteor, and the previous one of March 3, confirm the statement ('Observatory,' vol. iii. p. 129) that "during the first four days of March, fireballs have been very numerous." The epoch is a notable one for large meteors, and they should be especially looked for at this date in future years.

May 31, $9^h\ 8^m$. A fireball of unusual brightness was observed by Mr. C. Grover, at Mr. Peek's observatory at Rousdon. "When first seen the altitude was about 40°, and it fell nearly vertically, disappearing about 10° above the S.W. horizon. The path inclined a little from the vertical towards the N., and after falling about 20° there was a sudden pause and a deviation from the previous direction a little towards the N. Daylight was hardly gone, and the only objects visible were Mars, Jupiter, and Arcturus. At disappearance, the fireball was nearly half the size of the Moon, of a bluish colour and of intense brilliancy, fully three times as bright as Venus at the maximum. The duration was very brief, not more than 3 secs., and no train of sparks was seen."

June 14, $11^h\ 30^m$. A large meteor seen by Mr. W. R. Waugh, of the Manse, Portland. "It seemed to come from δ Ophiuchi and disappeared near γ Virginis. The head was comparatively faint, but the stream of light which followed was bright and wide, and of a deep orange tint. The length of apparition was about 1½ secs. Several telescopic meteors passed my field while observing with a 4½-in. O.G. They seemed to be moving pretty much in the same direction."

We are now entering upon a season which is the most productive and interesting of all to meteoric observers. Those who are fortunate enough to witness fireballs* should always register the apparent paths by the stars, and estimate their durations of flight. Such records become really valuable when they are approximately correct, and if more observers applied themselves to this branch we should soon acquire materials which must considerably enlarge our knowledge. A special effort ought to be always made to fix the *apparent directions* of such meteors as appear; other details, such as brilliant effects, coruscations, bursting, trains of sparks, streaks, or peculiarities of motion are of less significance, though we often find careful references to them while more important features are quite disregarded. Directly a meteor is seen the beginning and end points of its flight should be noted as precisely as circumstances allow, and an attempt made to assign its duration; the other facts of the display can be noted afterwards. I would be glad to receive such notices of these phenomena with a view to discussing them in the 'Observatory.'

Bristol, 1888, June 19. W. F. Denning.

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* The 'British Association Report' for 1878, and the 'Monthly Notices' for April 1884, contain some useful suggestions with reference to the observation of these bodies.

Selenographical Notes.

Letronne.—Among the many curved inflexions, resembling terrestrial bays, bordering the Maria, there are none that, in point of size and prominence, can compare with the expanded opening in the lofty mountain region north of Gassendi, termed Letronne,—a remarkable type of lunar formation, equally worthy of examination under a high or low Sun. Neison describes it as "a great bay on the border of the Oceanus Procellarum formed by the mountains of Gassendi,"—a statement which is true only in a topographical sense, for a glance at sunrise shows that, though closely associated with these highlands, it has a distinct individuality and displays characteristics which place it in the same category as Fracastorius and other large incomplete rings existing on the borderland between the low-lying and more elevated regions of the Moon. There are no indications of it having been scooped out by aqueous action, like many terrestrial bays; on the contrary, it is defined by a clearly-marked border, consisting of more or less lofty mountains concentric with the shape of the opening. Hence it seems to be part of a true ring, and not a mere re-entering curve or hollow in the escarpment of the Gassendi highlands, and has all the appearance of having originated in the same way as other large mountain-circles of the kind on the Moon.

At 8^h 30^m, on May 21, the morning-terminator lay a little west of Flamsteed, and though nearly the whole of the interior of Letronne, except where the shadows of the western border fell, was illuminated, only the loftier portions of the eastern wall were in sunlight, exhibiting a number of isolated bright segments which served to mark its position and extent; while the brilliant opposite flank, forming a projecting cape on the north like one of the horns of a crescent, was at this time a magnificent telescopic object under powers 284 and 340, which showed to perfection the details of its structure and the very complex character of the mountain region which extends between it and Gassendi. It was evident enough under these conditions that the west wall is a portion of a distinct ring, the elongated depressions (of which there are some interesting examples), the crater-rows, and subsidiary hills, lying west of the crest, being all roughly concentric with the curvature of the bay; while the hills and valleys beyond are, to use a geological term, unconformable to these features. It is the same as regards the eastern flank of the opening, though here the mountains are less lofty and of a somewhat different character.

From its situation Letronne is much fore-shortened, actually differing in shape only very slightly from a semicircle. Does it, like Fracastorius, Doppelmeyer, Le Monnier, and other bays, exhibit traces of a supplementary arc which, if in existence, would complete the ring? I think that anyone who carefully examines it at sunrise will admit that appearances at this time point very strongly in favour of a once entire formation. Extending from near the cape-like terminations of the bright east and west walls, and meeting at an obtuse angle about the place where the extreme northern side of the ring would lie if it existed, are two straight ridges which, though of no great altitude, are wide and conspicuous, and form a low barrier across the mouth of the bay, completely enclosing it, if the very narrow gap between the more westerly of these ridges and the northern end of the west wall be left out of account. The formation at this stage has in fact many of the distinctive characters associated with the Fracastorius type, and it is hard to resist the conclusion that it was once complete, but has suffered partial destruction on the side abutting on the Oceanus Procellarum,—the ridges observed representing the sole remains of the northern section of the circumvallation. In lieu of direct observation, a reference to Schmidt's map alone is sufficient to show that there is good ground for this supposition. He represents Letronne as an ovate-shaped enclosure, the bright existing ring forming the larger, and two ridges the more pointed end.

Between 8^h 30^m and 9^h 30^m on May 21, the large crater B on the western side of the formation was partially hidden by the shadow of the loftiest section of the west wall, which, according to Mädler, culminates in a peak some 3000 feet high. North of this there is a narrow break in the border and then another peak, ρ (Neison, Map xii.), on the promontory, about 1000 feet lower,—the shadows, as well as others more acute towards the south, being very clean cut and black. Within the enclosure, the most noteworthy objects were three brilliant little mountains near the centre, with a fourth, less bright, on the east. Mädler and Neison show a solitary mountain, A, which is probably intended to represent these objects; but, if so, it is placed considerably too far to the north of their true position. Schmidt draws four hills here close together. The group of three appears to stand on a ridge which, commencing a short distance south of them, follows a curved course (convex to the west) to Flamsteed A, crossing on its way the eastern ridge pertaining to the ruined border. These features, at sunrise, render the resemblance of Letronne to an imperfect ring still more striking, as representing the relics of a central elevation.

In a line between the bright crater Letronne A and Wichmann, and rather nearer the latter, lie several little mounds, among which are two small craters, open on the north and south, and a minute crater west of them. The object marked *f* by Schmidt is shown near the site, but is drawn much larger than either of these features. THOMAS GWYN ELGER.

Kempston, Beds, 1888, June 19.

Note on some Investigations of the Accuracy of the Paris Photographs.

THE papers of Thiele and Scheiner recently published in the 1st part of the Bulletin of the Astrophotographic Congress may be safely accepted as absolute proof that the photographs of MM. Henry are capable of measurement with all the accuracy which is necessary in many of the most refined operations of practical astronomy, provided that due regard is paid to the systematic errors which may arise from distortion of the film and of the optical field. The results of Professor Vogel (Astron. Nach. 2833) show clearly how perfectly errors produced by distortions of the film can be eliminated by aid of the réseau, and how crude must be the results if the réseau is not employed.

It is peculiarly satisfactory to note that the accuracy of measurement is not diminished by length of exposure, but, on the contrary, that Dr. Scheiner finds, within the limit of exposure which will probably be required for 11th magnitude stars, that there is a sensible increase of accuracy by increase of exposure. This appears to be explicable as follows: any one who has observed with a heliometer knows that when the images of two stars situated one or two degrees apart are brought together, these images, except in very rare intervals of steady definition, are in continual movement with respect to each other, on account of small changes in differential refraction* which are continually taking place over comparatively limited areas, and these abnormal conditions are frequently persistent in one direction for very sensible periods. What we obtain therefore on the photographic plate is not the steady impression of a minute disc of light on the same precise spot of the film, but a continuous series of impressions of a point of light over an area which may be one or more seconds of arc in diameter, even if we suppose the axis of the telescope throughout the exposure to be constantly directed to the true middle point between the two stars under consideration. The resulting figure of the photographic image (if we further assume for the moment the non-existence of photographic irradiation †) is an impression of the integration of the whole of these successive impressions during the period of exposure. Or, in other words, the star's image will trace out upon the sensitive plate a line, which irradiation will tend to fill up from a line into a tint, and the blackness of the tint will be proportional to the number of lines in a unit of area.

If the vibrations are on the whole symmetrical about a mean point, the result will be an intensely black central disc very rapidly fading at the border, which is practically what we find. But if some systematically abnormal condition of the differential refraction has occurred during a considerable proportion of the exposure, the resulting relative places of the two star-images will not be identical with their true mean relative positions. The probable amount of this error will evidently depend on the relation of the duration of the abnormal differential disturbance to the duration of the exposure. Thus, long-exposed pictures of stars must be less liable to such errors than plates of very short exposure. (There are of course practical limits to advantageous length of exposure; these are imposed by the enlargement of star-images and by the corrections for refraction becoming too great, or being non-coincident with the mean instant of exposure.) Further, when it is desired to obtain the best results, MM. Henry, and indeed all experimentalists, have found the necessity for continually correcting the clock-motion by minute movements of the slow motions in right ascension and declination. This necessity arises partly from changes in the refraction, partly on account of small errors in the pivots of the polar axis or in the adjustment of the equatoreal, or partly from errors in the teeth or screw of the driving arc, or from errors in the rate of the driving-clock. Now it is clear that before any correction should be applied to the slow motions the observer must satisfy himself that such correction is necessary, and the amount of error arising before correction will depend on the state of the atmospheric conditions of definition and on the optical power and perfection of the guiding telescope. There are only two circumstances in which hand-correction will produce no systematic error in the relative positions of the photographed stars, viz.:—

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* The expressions *change in differential refraction* or *abnormal differential refraction* are here employed to express the local changes of refraction which affect the apparent relative distance and position-angle of two stars in the same field, as distinguished from those changes in the general density of the air by which the mean apparent position of the middle point between two stars would be affected.

† The effect of photographic irradiation (so called) is merely a tendency to fill up the spaces between contiguous images, and does not affect the form or mean position of the photographed disc.

a. If the stars are equally bright, or if both stars are sufficiently bright to produce an instantaneous visible impression on the photographic plate, because in that case all displacements of the axis of the telescope from its mean position are equally recorded by both stars, and if the hand-corrections are made unsymmetrically from a mean point the effect is only to produce a small elongation of the disc common to both stars.

b. If the hand-corrections are non-systematic (that is, if they are of a purely accidental character), they will tend to group themselves symmetrically about a mean point, and, according to the theory of probabilities, this symmetry will be the more perfect the longer is the exposure.

The imaginary case *b* is not that which occurs in practice, for on account of refraction &c. the star observed in the guiding telescope has a tendency to depart from the cross webs in a constant direction. Under these circumstances if one star is bright and the other so faint that a short exposure produces no visible trace on the film, we shall have the image of the brighter star elongated in the direction of the systematic tendency to depart from the cross webs of the guiding telescope, and in the case of the faint star a perceptible impression will only be formed on the plate very near the mean or normal point which the wandering point of light most frequently crosses. All the preceding considerations point to the desirability of practically testing the accuracy of measurement of the relative positions of bright and faint stars on the photographic plate[*]. The images of bright stars, after an exposure of a few minutes, become too large for accurate bisection, and to obviate this difficulty in parallax-researches it has been proposed to employ a small shutter, which would enable the observer to expose the bright star only for an instant at the beginning and end of the exposure. But, for the reasons stated in the preceding remarks, it seems probable that considerable accidental errors will thus arise, and until the point has been tested by experience Thiele's conclusion is premature, viz. "que nos observatoires devront remplacer le plus tôt possible les grands réfracteurs à micromètres et les héliomètres par des réfracteurs photographiques à grande distance focale." For the observation of comets the ordinary refractor is still essential, and for the observation of the parallax of the brighter stars, the observation of moving objects (such as the minor planets for parallax or stars and lunar objects for determination of the parallactic inequality), the heliometer, so far as all experience goes, is still the instrument *par excellence.*

On the other hand, for the investigation of the internal motions of star-clusters, or the parallax of fainter stars which have large proper motions, Thiele's and Scheiner's results are conclusive, and prove that if the effects of distortion of the film are eliminated by use of the réseau, the photographic method will afford a close approximation to the accuracy of heliometer observations, with the advantage of accomplishing more work in a given time. The accuracy which is possible in the case of faint stars arises not only because their small discs are more suitable for accurate bisection, but because deviations of the optical image from the mean place on the film, produced by abnormal refraction, or other cause of short duration, are not visibly registered, and only those impressions which traverse frequently the same point on the film combine to make up the visual image.

There is a kindred illustration in Pickering's photographed spectra of stars, where the spectral lines are shown even more sharply than they are usually seen by the eye, simply because those vibrations of the image which depart far from the mean point (and which, according to the theory of probabilities, seldom again occur precisely in the same way) are not recorded.

More recently we have Vogel's exquisite determinations of the motions of stars in the line of sight by photographic observations of their spectra, where he finds that unsteadiness of the definition to an extent sufficient to render eye observations impossible has no practical influence on the sharpness of the photographic spectra. In this case the explanation is still more complete, viz. that the slit of the spectroscope being made very narrow, all vibrations of the atmosphere simply cause more or less of the light of the star's

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[*] There is of course the additional question as to whether the possible non-symmetric rings of bright stars near the edge of the plate may produce displacement of the photographed images of bright stars different from those faint stars whose rings may be too faint to create a visible impression.

image to fall outside the slit (and therefore the necessary exposure may be a little prolonged), but they in no way affect the sharpness of the resulting picture.

Thus photography, in several very remarkable instances, appears to offer a satisfactory mode of escape from one of the practical astronomer's chiefest bugbears—unsteady definition.

David Gill.

The Photographic Chart of the Heavens.

Mon cher M. Common,—

Permettez moi de répondre en quelques lignes à votre nouvelle notice de l'*Observatory*.

Il y a en effet un léger malentendu entre nous : par ma dernière lettre je voulais seulement appeler l'attention des astronomes sur la nécessité de rechercher un meilleur procédé pour la mesure de clichés que le projet de M. le Dr. Gill, si on refusait d'adopter le sien.

Mais je n'avais nullement l'intention de discuter sur l'utilité du catalogue. Les décisions fondamentales ont été prises par le congrès, et le comité permanent n'a d'autre fonction que de les exécuter.

Vous vous rappelez d'ailleurs que la question de ce catalogue a été discutée et mûrement etudiée par le congrès et que la décision a *été prise à l'unanimité.*

Comme vous avez pris part à ces discussions, vous pouviez faire alors plus utilement les objections que vous faits aujourd'hui, tandisque le procès verbal constate que vous n'avez fait aucune opposition et que votre vote a été favorable puisqu'il y a eu *unanimité* dans la décision prise.

Maintenant, vous adressant ce moi, vous me demandez de vous donner mes arguments en faveur du Catalogue. Comme président du Bureau permanent, je n'ai rien à répondre qu'à dire que nous devons éxécuter les décisions prises par le congrès ; mais si vous voulez non opinion personnelle, je puis vous avouer bien franchement que quand j'ai pensé à utiliser ce merveilleux progrès de la photographie pour fixer a jamais l'état du ciel actuel, je n'avais nullement pensé à la construction d'un catalogue. J'aurais trouvé tout-à-fait superflu de traduire la carte du ciel en innombrables colonnes de chiffres, quand ayant cette carte entière, sous les yeux on pouvait recommencer en tout temps en tous lieux, toutes les mesures et les comparaisons dont en pourrait avoir besoin. La photographie me paraissait mieux appropriée à déterminer les variations de position relative des étoiles que leur position absolue.

Mais je me hâte d'ajouter qu'étant beaucoup plus marin qu'astronome je me trouvais trop incompetent devant les savants si éminents qui déclaraient ce catalogue indispensable, pour essayer même de combattre leur opinion, et c'est par déference pour eux que bien que je ne visse pas clairement l'utilité de leur demande je n'ai pas hésité à voter comme eux, puisque cette décision n'entravait en rien l'œuvre principale de la photographie du ciel; comme vous avez aussi voté comme eux bien que votre opinion fut contraire il me semble que nous sommes à peu près dans le même cas tous les deux.

Tous les astronomes doivent se préoccuper aujourd'hui de la meilleure manière d'utiliser les immenses et si précieux matériaux que la photographie va mettre bientôt à leur disposition, soit par des catalogues, soit par des reproductions sur verre ou sur papier pour vulgariser ces documents ; mais en réalité c'est là pour moi une question secondaire, puisqu'on aura toujours le temps de la résoudre et de l'étudier de nouveau quand la carte sera faite.

Le point capital auquel nous devons tous attacher la plus haute importance est l'exécution immédiate de cette carte et des deux séries de clichés demandés par le congrès; la question secondaire d'utilisation, les discussions auxquelles elle peut donner lieu ne doivent apporter aucune entrave aucun trouble dans l'exécution de l'œuvre principale; avec les 15 ou 16 observatoires qui vont y prendre part nous commencerons le travail l'année prochaine et nous le pousserons aussi activement que possible de manière à ce qu'il soit terminé au plus tard en 3 ou 4 ans. Je ne suis nullement inquiet sur la manière dont les astronomes de l'avenir sauront utiliser l'œuvre capitale que nous leur léguerons.

Quant au Bureau central de mesures, je le crois très utile, et on pourrait le faire certainement moins coûteux que celui de M. Gill.

Veuillez agréer, mon cher M. Common,
L'assurance de mes sentiments très affectueux,
E. MOUCHEZ.

Observatoire de Paris, le 11 juin, 1888.

[*Note by* MR. COMMON.—I rejoice to find from the above letter that after all, in every important particular, Admiral Mouchez is quite in agreement with the views expressed by Mr. Turner and myself.

There is but one question that I should like to answer and one that I should like to ask at present. Admiral Mouchez blames me for not having made the objections at the Congress that I now make. I can only say that the question of such a catalogue as that lately proposed was never before the Congress in a definite form ; had it been so and voted upon, it is pretty certain that the voting would not have been unanimous. It is on this point that there is a direct difference of opinion between us ; and although I do not think much will be gained now by discussing such matters, still I should feel grateful to Admiral Mouchez if he will tell me exactly what resolution (or resolutions) he considers contains the decision of the Congress on the question of the catalogue. Of course we must confine ourselves entirely to the resolutions themselves, without reference to the opinions expressed by individual members of the Congress.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Mars.

Messieurs,—

Je crois que les lecteurs de l'*Observatory* verront avec intérêt un résumé des résultats que j'ai obtenus jusqu'aujourd'hui (4 juin) en observant Mars avec mon équatorial de huit pouces de Grubb. J'avais pour but, dans ces observations, de vérifier la parfaite exactitude des belles cartes de M. Schiaparelli, exactitude que j'avais toujours défendue contre les attaques d'une incrédulité très excusable d'ailleurs. Quoique le temps ait été souvent très défavorable (je n'ai obtenu une image bien satisfaisante qu'une fois ou deux tout au plus), quoique la déclinaison australe de Mars ait souvent empêché l'emploi de grossissements supérieurs à 280 fois, quoique la distance de la planète ait rendu certains détails fort difficiles à voir, quoique certaines régions soient restées complètement inexplorées, j'en ai vu assez pour proclamer que M. Schiaparelli le premier et seul jusqu'ici avait vu l'entière vérité.

Je commencerai par donner la liste des régions de la carte que j'ai eu l'occasion d'observer et de vérifier et, si, dans cette énumération, je comprends des objets déjà connus antérieurement sous d'autres noms, j'entends que je les ai observés en conformité complète avec la carte de Milan :

Mare Sirenum, Atlantis, Sinus Titanum.
Mare Cimmerium, embouchure du Laestrygonum, continuée par l'Antæus, Hesperia.
Erebus, Cerberus, Trivium Charontis, Styx, Hyblæus, Eunostos, Propontis, Herculis pons.
Elysium, Ætheria, Triton, Lethes, Syrtis minor, Tyrrhenum mare, Libya, Thoth, Nepenthes, Alcyonius.
Syrtis major, Nilosyrtis, Boreosyrtis.
Orontes, Phison, Astusapes.
Protonilus, Ismenius lacus, Deuteronilus, Callirrhoë.
Sinus Sabæus, Edom promontorium, Aryn promontorium, trace confusé d'Hiddekel et Gehon confondus, Deucalionis regio, Indus se prolongeant dans l'Oxus.
Margaritifer Sinus, Auroræ Sinus, Pyrrhæ et Protei regio, Ganges, Argyre.
Niliacus lacus, Achillis pons, Acidalium mare, lacus hyperboreus *, Nilo Keras, Lunæ lacus, Nilus, Ceraunius, Tanaïs Tempe, Nectar, Solis lacus, Agathodæmon, Tithonius lacus.

J'ai été frappé, comme M. Perrotin, de la pâleur de Tyrrhenum mare, mais j'ai parfaitement vu Libya qui ne m'a pas semblé aussi sombre qu'à cet astronome ; l'obliquité sous laquelle se présentent ces régions est peut-être cause de leur changement d'aspect, mais la

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* Ce nom a été donné par M. Schiaparelli depuis ses dernières publications sur Mars à un petit lac situé entre Mare Acidalium et la tache polaire nord.

Libye ne m'a semblé ni disparue ni inondée; si Tyrrhenum mare a pâlé, le Nepenthes est très noirs en revanche. Je n'ai pas vu non plus le Lacus Moeris.

Quant au canal ou un fil noir que M. Perrotin a observé dans la tache polaire, je l'ai observé ici depuis le 12 mai; il semble effectivement d'abord établir une division dans la calotte neigeuse du pôle; en regardant avec grande attention, j'ai vu que la région neigeuse qui est à son extérieur était moins blanche et moins brillante que la tache polaire elle-même. Je crois qu'il faut considérer cette région neigeuse comme une petite terre hyperboréenne, séparée de la calotte neigeuse polaire proprement dite par le filet noir en question. J'ai vu cette petite terre tourner autour de la calotte polaire. La première impression du filet noir était cette d'un point noir qui se serait trouvé dans la calotte polaire et rappelait des observations de Mitchell produites probablement par la même cause; mais avec un peu d'attention on voyait très bien le petit canal noir séparant cette petite tache blanche de la tache polaire véritable. Cette petite terre, par son voisinage du pôle, est comparable au petit *lacus hyperboreus* situé sous *Mare Acidalium*, contre la tache polaire.

J'ai vu aussi, sur le prolongement de l'Erebus, au bord gauche ou occidental (image renversée) trois points brillants, lorsque le Trivium Charontis est à mi-chemin du méridien central, dans la moitié droite ou orientale. Ces trois points devenaient de plus en plus blancs et brillants en approchant du bord où ils finissaient par déborder comme la tache polaire, par irradiation. Je continue encore utilement les observations (4 juin).

Je suis, Messieurs,
Votre bien dévoué,
F. Terby.

Louvain, 1888, June 4.

η Argûs and W. B. XIII. 595.

Gentlemen,—

After making some telescopic comparisons of R Carinæ on the evening of the 19th instant I turned the instrument on η Argûs for the first time since 1887, April 23, and was immediately struck with the clear and sparkling character of its rays as compared with the dull light emitted by it for some years past. I proceeded at once to compare the star with its neighbours, and repeated these comparisons under favourable conditions on the following two evenings. Careful comparisons with Sir J. Herschel's Cape stars contained in Gould's List on page 256 of the 'Uranometria Argentina' showed that the variable had increased fully half a magnitude since my last observations in April 1387. According to Gould's scale, on 256 of the work just cited, it may now be rated as a star of the 7·0 magnitude. Is this sudden increase after such a long period of comparative rest to be regarded as premonitory of another magnificent outburst of this remarkable variable? As a help to

the solution of this question the star will be systematically watched here for the future.

While searching for the planet Sappho in March last I had occasion to determine the positions of several small stars relatively to Lalande 25284, a star of the 8th magnitude. One of these, a star of the 9th magnitude, I found to be $8^{s}\cdot30$ east and 26″·8 south of the comparison star. On reference to Weisse's Bessel at the Sydney Observatory, I found that it contained only one star (No. 595 =9 mag.) close to this position, namely $7^{s}\cdot88$ west and 7″·9 north of Lalande 25284 or W.B. 599. Is the discrepancy between the differential declinations for 1825 and 1888, amounting to nearly 35″, due to an error in the single observation of Bessel or to proper motion? A recorded observation for an intermediate epoch would throw light on this question.

Windsor, N. S. Wales,
1888, May 22.

Yours faithfully,
JOHN TEBBUTT.

[The W.B. star is No. 3781 of Schönfeld's Zone −14°; and the place there recorded seems to show that the W.B. place is in error, and that the discordance is not due to proper motion. The correction is pointed out by Prof. C. H. F. Peters in his paper "Corrigenda in Various Star Catalogues."—EDS.]

Narrien and the Observations of Eudoxus of Cnidus.

GENTLEMEN,—

Very high praise is awarded to Narrien's 'Historical Account of the Origin and Progress of Astronomy' in the notice (it does not amount to a biographical notice, which they were unable to procure) of him in the Annual Report of the Council of the R. A. S. for 1861. The work was published in 1833; and it is to be regretted that it never reached a second edition, which would have enabled the author to correct several errors of inadvertence into which he fell. As instances of these I will just mention two remarks on p. 406, where we read that "the Gregorian reformation of the calendar is now adopted in all Christian states," and that by that calendar "it was regulated that the intercalary day, which according to the Julian calendar enters in every fourth year, should be omitted in every year forming a complete hundred, except in those years which constitute complete thousands, when it was to be retained." It is odd that it did not occur to Prof. Narrien that, had this really been the Gregorian rule, the Parliament of Great Britain need not have omitted in 1752 one day more than Pope Gregory XIII. had done in 1582.

But one mistake in the work in question cannot be the result of mere inadvertence. Sir G. Cornewall Lewis first pointed it out many years ago; and as it may mislead some of those who read it, perhaps you will allow me to call attention to it. At p. 248 Narrien writes:—"Strabo relates that, in his time, there were shewn, in the prefecture called Litopolitana, near Heliopolis, certain caves which had been used for observing the motions of the celestial bodies; and he remarks that similar caves were in existence near Cnidus in Asia. Such excavations, if sufficiently deep, would permit the planets and principal fixed stars to be seen in the daytime; and, if made in the plane of the meridian, would be well adapted for witnessing their transits over that plane, and, consequently, for ascertaining immediately, with instruments, their right ascensions and declinations." I fear we have here a description of an approximate method of taking complete meridian observations which was quite beyond the powers of the astronomers of the time in question, not to mention the impossibility of seeing "the planets and principal fixed stars" in the manner alleged. But certainly Strabo is not responsible for any such statements. The reference is to lib. xvii. cap. 1, § 30, of his Geography; and the word translated "cave" by Narrien is *σκοπή*, an observatory or observing-station. The passage runs thus:—*δείκνυται γὰρ σκοπή τις πρὸ τῆς Ἡλίου πόλεως, καθάπερ καὶ πρὸ τῆς Κνίδου, πρὸς ἣν ἐσημειοῦτο ἐκεῖνος* [*i. e.* Eudoxus] *τῶν οὐρανίων τινὰς κινήσεις.*

As Sir G. Cornewall Lewis remarks, apparently the only possible way of accounting for this error of Narrien is that he referred to a Latin copy of Strabo, and mistook the word, translating *σκοπή* (specula) for specus (a cave). There can be no reasonable doubt that Eudoxus observed on hills and not in caves.

Yours faithfully,

Blackheath, 1888, Mar. 27. W. T. Lynn.

OBSERVATORIES.

Oxford University Observatory.—Prof. Pritchard presented his thirteenth annual report to the Board of Visitors on June 6. From this report we learn that in addition to the statutable lectures, others of a more elementary and quasi-public character, expressed, as far as possible, in untechnical language, were so successful that it is proposed to give a more extended series on recent speculations as to the origin of the Cosmos from meteoric collision, and cognate matters. Mr. Nasmyth has presented his celebrated picture map of the Moon (6 feet in diameter) to the Observatory. The transit instrument has completely fulfilled the expectations of its usefulness. Two silver-on-glass mirrors of the same aperture but of very different focal lengths were examined for the Photographic Committee of the Royal Society to ascertain the practical effects of focal length on the photographic field, and it was found that mirrors (particularly those of short focal length) are comparatively unsuitable for charting the heavens; but that there is no doubt of their capacity for accurate delineation of small portions of the sky, and for such operations as those connected with stellar parallax or

the charting of the Moon. The De La Rue reflector is in excellent order. Dr. De La Rue having generously provided the funds necessary for a photographic telescope of the pattern suggested at the Paris Conference, the large equatoreal has been sent to Sir H. Grubb to have the necessary alterations made, and the photographic telescope attached to it. During the past year continuous attention has been devoted to the photography of small portions of the heavens with a view to determining the parallax of certain selected stars. The method was first tried on the components of 61 Cygni, as the parallax of the point midway between the two stars had been determined with presumably great accuracy by Bessel in 1838, and thus the old and new methods could be effectively compared. The general agreement of the two results, and the remarkable consistency of the individual photographic measurements, satisfied Prof. Pritchard not only of the great convenience, but also of the unimpeachable accuracy of the method. The field of operations was then extended, and it is expected that by the end of this year the computation of the parallaxes of 12 stars will be completed. The results already obtained are :—

61^1 Cygni	0″·4289 ± 0″·0180.	μ Cassiopeiæ	0″·0356 ± 0″·0250.
61^2 Cygni	0 ·4353 ± ·0152.	Polaris	0 ·052 ± ·0314.

and provisionally from six months' observations :—

α Cassiopeiæ	0″·072 ± 0″·042.	γ Cassiopeiæ	< 0″·050 ± ″·047.
β Cassiopeiæ	0 ·187 ± 0 ·039.		

Prof. Pritchard acknowledges the skilful and sedulous manner in which his assistants, Messrs. Plummer and Jenkins, have carried out the work above referred to.

PUBLICATIONS.

Harvard College.—Prof. Pickering has issued a volume of the collected minor papers published by the various members of his staff during the last ten years. There is nothing essentially new about the volume except the binding, which, however, deserves a word of praise. Prof. Pickering kindly offers to supply separate copies of such papers as he has still left to those specially interested in any particular subject.

We have received also the Second Annual Report of the work known as the Henry Draper Memorial, which has been considerably extended during the past year by the continued liberality of Mrs. Draper, to which we have already referred, see ('Observatory,' No. 134, p. 161). We notice that systematic measurements of the sensitiveness of the plates, of the clearness of the sky, and of the definition, have been undertaken, so as to diminish considerably the annoyances of uncertainty of exposure which have hitherto been so persistent in astronomical photography.

Handbook to the Lick Observatory.*—This little volume of 135 pages contains a great deal of information and a poem. In view of the interest naturally excited by the Great Refractor, and the romantic situation of the Observatory, the Director apparently anticipates a crowd of visitors to Mt. Hamilton; and is amiably endeavouring to do all he can for them on Saturdays in the hopes that they will leave him leisure for his regular work during the week. The Handbook which he has compiled for them is admirably suited to its purpose, and pleasantly written. We have an account of James Lick, his life, bequest, death, and burial under the pier of his great telescope; a history and description of the Observatory and its instruments; articles on the work of an observatory, telescopes, astronomical photography, and the chief observatories of the world; valuable travelling and other directions to tourists; and the aforesaid poem to the as yet unmounted lens of the 36-in., which is apostrophized as a 'Mysterious Eye.' There are some good illustrations.

The Satellites of Saturn.—The first number of a new series of publications called 'Supplements to the Pulkowa Observations,' which will, generally speaking, contain short and complete investigations, consists of an interesting memoir by Prof. Hermann Struve on the Outer Satellites of Saturn. He discusses his own observations, made with the 15-inch refractor in the years 1884–1886 on Iapetus, Titan, Rhea, and Dione, with a view to correcting the elements of these satellites, and also the values of the mass and ellipticity of Saturn. The mass of Saturn was found by Bessel to be 1/3501·6 ± 0·77, or, with a slight correction indicated in the present paper, 1/3502·5. Prof. Asaph Hall, on the other hand, obtained the value 1/3481·3 ± 0·54. Herr Struve considers the rather large discordance between these values due to systematic error in measuring the distance of a satellite from a limb, and his own observations consist entirely of comparisons of one satellite with another, either by differences of R.A. and N.P.D., or of distance and position-angle. His resulting value of the mass of Saturn agrees closely with Bessel's, being 1/3498. The correction of the elements has been carefully and laboriously carried out by the method of least squares. A similar memoir, dealing with measures made on the inner satellites, and using the 30-inch refractor, is to follow within a year or two.

The Paris Catalogue of Stars.—This, the first star catalogue of the Paris Observatory, is now in course of publication. Up to the present time two volumes of the work have been published; one, the first of the series of volumes of the catalogue, properly so called, gives the places of the stars in the first six hours of R.A., the other contains the separate reduced observations of the same stars. Each series when complete will extend to four volumes. The observations of the stars contained in the catalogue were made with the meridian instruments of the Paris Observatory, five in number, between 1837 and 1881, and as this period was considered too long to allow of the reduction of all the observations to a single epoch, three epochs have been chosen as follows:—observations made between 1837–1853 have been reduced to 1845·0, those made between 1854–1867 have been reduced to 1860·0, and those made between 1868–1881 have been reduced to 1875·0. Since 1854 the meridian work at Paris has been almost exclusively confined to the reobservation of Lalande's stars, and the volumes now published give the places of 7245 stars depending on 80,000 observations of right ascension and of declination; the complete work ought therefore to furnish the places of about 30,000 stars. The probable error of a single observation, estimated by its discordance from the mean, has been very thoroughly discussed in the Introduction to the catalogue; in R.A. it is found to be $\pm\sqrt{(0^{s}{\cdot}049)^2+(0^{s}{\cdot}041\sec\delta)^2}$, which at the equator becomes $\pm 0^{s}{\cdot}064$, and in declination the value is $\pm 0''{\cdot}71$. There does not appear to be any sensible difference in the probable errors of the observations made during the three periods into which they have been divided. As is perhaps inevitable in the discussion of observations extending over so many years, the percentage of observations too discordant to be included in the mean is rather high: thus from an examination of the first hour of R.A. it appears that about 50 observations out of 12,000, or 0·4 per cent., have been rejected. We are not told on what principle this rejection is based, and there appears to be a little inconsistency in its application; thus in the case of No. 482, with polar distance 44°, two observations differ by $0^{s}{\cdot}67$, and they have both been retained with a note to the effect that the difference is inexplicable. Again, in the case of No. 1429, with polar distance 52°, two observations differ by $0^{s}{\cdot}58$, and they have both been retained with a note to the effect that the discordance is probably due to the fact that in each case another star was simultaneously observed in the same field. In other cases such discordances as these have been considered excessive, and the discordant observation has been rejected. In reducing the observations to their respective epochs Le Verrier's precessions have been used, and in the catalogue the first and second terms of precession for 1875 computed from Le Verrier's formulæ are given. In no case has proper motion been applied to the catalogue places, but a discussion of the proper motion of a large number of stars, by M. Bossert, is given in the Introduction. M. Bossert has made a complete comparison of the Paris places with Lalande's places, and where these showed a discordance of over 1^{s} in R.A. or 10″ in declination, he has consulted the other available star catalogues in order to decide whether the discordance is due to proper motion or not. There are some cases of outstanding discordance which remain unexplained. A comparison has also been made between the Paris places and those of Auwers' Bradley. This of course in its incomplete state (extending only from 0^{h}–6^{h} of R.A.) is of little value except for determination of the proper motions of individual stars. In neither of the volumes before us are we given any information as to the places of fundamental stars on which the Paris positions depend, or whether any attempt has been made to reduce the latter to a homogeneous system. This is the more to be regretted as the polar distances of the Paris Catalogue are not independent, but the "polar points" of the mural circles and meridian circles have been deduced from the assumed places of fundamental stars. An attempt has been made to get rid of some of the systematic errors thus introduced by the discussion of observations of circumpolar stars observed above and below the pole; but if the Paris Catalogue is to take rank as anything higher than a collection of places of faint stars, useful as comparison stars, for comet or minor planet observations or similar work, it is necessary that full information on these points should be given to the astronomical public, as we hope it will be when the complete work is published.

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* By Edward S. Holden, Director. Published by the Bancroft Company, San Francisco. Price not mentioned.

The work of formation of the catalogue has been confided to MM. Gaillot and Bossert, who have spared no pains to make it accurate, especially in the difficult matter of identification of stars where observations in right ascension and declination have been made with different instruments, as has been the case at Paris.

A. M. W. D.

NOTES.

COMET SAWERTHAL.—The sudden increase in brightness of this comet has been noticed by many observers. Dr. Copeland read a paper on the subject at the last meeting of the R. A. S. on June 8, giving estimations of increase in brightness of the nucleus only; this increase he recorded as two or three tenths of a magnitude, but he called attention to the possible influence of the strong twilight at Dunecht on the estimations. In the Astron. Nach. No. 2843, Herr F. Blumbach estimates the magnitude of the nucleus at 9–10 on May 19 and 7·8 on May 22.

Dr. J. Franz, of Königsberg, gives the increase in brightness as 3½ magnitudes and the maximum brightness as 5·8. Herr A. Kammermann gives 2 or 3 magnitudes and 5·5 for these figures.

In No. 2844 Father J. Fenyi, of Haynald Observatory, gives a very complete list of magnitudes of the nucleus as follows:—

May 20	$9^{m}\cdot3$	May 23	$6^{m}\cdot8$
„ 21	$7\cdot8$	June 2	$7\cdot3$
„ 22	$6\cdot8$		

The increase was thus estimated at 2½ magnitudes. A set of good sketches is added, showing the change to a winged appearance which accompanied the blazing-up.

Dr. Wutschichowski also writes on the subject; but the table of his results has been necessarily postponed. The comet soon faded again to its former faintness. Owing to bad weather and the moonlight, it has not lately been seen at Greenwich.

Ephemeris for Berlin Midnight *.

1888.	R.A. h	m	s	Decl. °	′	Log *r*.	Log Δ.
July 1 ..	1	1	42	+48	3·7	0·3009	0·3212
5 ..		4	9	48	56·0	0·3127	0·3247
9 ..		6	1	49	45·7	0·3241	0·3278
13 ..		7	18	50	32·8	0·3352	0·3306
17 ..		7	56	51	17·2	0·3459	0·3331
21 ..		7	57	51	58·8	0·3563	0·3353
25 ..		7	19	52	37·4	0·3664	0·3372
29 ..		6	0	53	12·6	0·3762	0·3389
Aug. 2 ..	1	4	1	+53	44·2	0·3857	0·3405

MINOR PLANETS.—No. 272 has been named Antonia. That referred to last month as No. 278 has proved to be No. 116 Sirona, the somewhat large discordance between observation and the ephemeris being due to the omission of perturbations in the calculation. Herr Palisa's discovery on May 16 thus becomes No. 278.

A CLOCKWORK TRANSIT INSTRUMENT.—Since the recognition of personal equation in transit observing, a large amount of ingenuity has been spent in trying to devise some means of evading the correction necessitated by ordinary methods of procedure. The magnitude of the correction is certainly at first sight somewhat alarming: when we find that results which we should like to have accurate to hundredths of a second are obtained by crediting one observer with a capability of recording several tenths of a second before another, we are inclined to feel a slight mistrust of the human organization as a recording agent. The recent development of photographic processes has naturally suggested one or more methods for getting rid of the human observer by taking photographic transits, the general principle being to interrupt the photographic trace automatically from the clock.

Herr Repsold has recently proposed † a partially automatic method of recording transits which does not, however, depend on photography. The transit is mounted so as to be virtually an equatoreal, with a small motion only in hour-angle near the meridian. A star just before transit is brought into the centre of the field of view and the driving-clock started, so that the star remains steadily in the same part of the field, and its position in the field may be observed with the R.A. micrometer. Meanwhile the telescope is following the star up to the meridian, and on reaching the meridian the clockwork is automatically disconnected and a record made on the chronograph sheet. An observation of R.A. is thus assimilated to one of N.P.D., consisting of two parts—first, the record of position of the object in the field, and, secondly, of the position of the telescope, the latter being here automatic. It is not to be expected that we should get rid of old errors without introducing new ones; and we should think that the errors of clock-driving here introduced would be liable to be troublesome. Personality is not, of course, entirely eliminated; there are still the micrometric measurements and the reading of the chronograph sheet. It is true that personal errors in such measures as these are of a much smaller order than in ordinary transit observing; but many investigations have shown that transit personal equations though large are not very variable; it is the change in personal equation which really concerns us, and these changes are for ordinarily good observers very little greater than such errors as would be introduced by automatic arrangements. There are some inconveniences about a chronometer with a large rate, but it may be a much better timekeeper than another with a mean rate zero.

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* Astron. Nach. No. 2838.

† Ibid. No. 2828.

ARRANGEMENTS are being made for a redetermination of the longitude Paris—Greenwich. The last determination was made in 1854 under the direction of Le Verrier and Airy. The latter had been in correspondence with Arago on the subject three years earlier; but Arago died before the project was completed. It is a curious coincidence that General Perrier, who proposed the present redetermination to the Astronomer Royal, should also not have lived to carry it out. The observers in 1854 were Faye and Dunkin; the observations both of time at each end and of signals between the stations were made by eye and ear—the object observed in the latter case being a galvanometer needle (or rather a pair, one at each end), deflected simultaneously by an electric current. The separate results corrected for personal equation were:—

m s	m s	m s	m s
9 20·53	9 20·51	9 20·66	9 20·71
·72	·69	·64	·71
·67	·69	·56	·64
·58	·66	·61	·62
·62	·66	·66	·61
·62	·61		

Mean $9^{m}\ 20^{s}{\cdot}64$.

This implied a correction of a whole second to the previously accepted value. In the operations as at present contemplated the whole work is to be done chronographically; four observers, M. le Commandant Bassot and M. Defforges from Paris, and Mr. Turner and Mr. Lewis from Greenwich, are to take part in the work, so as to have a double check on the personal equation. The point of observation at Paris is not to be the Observatory as before, but the Geodetic station at Montsouris, whose position, however, with reference to the Paris Observatory is known to an inch or two, and which has been connected with several other important European points.

The French observers, with M. Loewy of the Paris Observatory and the Bureau des Longitudes, recently visited England to discuss the plan of operations with the Astronomer Royal, and were welcomed by several leading astronomers.

MR. J. H. BRASHEAR has recently called the attention of astronomers and instrument-makers to a most important matter in practical astronomy—the desirability of establishing standard sizes for the various parts of instruments, so that those of different apparatus may be interchangeable. In a paper read before the American Association for the Advancement of Science, Mr. Brashear says :—"How many of the parts of an astronomical or physical instrument should be made interchangeable, I am not now willing to say, but every worker with the telescope, spectroscope, or other instrument for physical research will bear me out in this fact, that there is a sore need of standard dimensions in many of the parts of our apparatus. Indeed, our President, Professor Langley, was one of the very first to call my attention to the matter, and suggested that it would be a very excellent plan for this Association to appoint a committee to discuss, and, if possible, decide upon some standard dimensions of the more important parts which should, at the earliest date, be made interchangeable. I might urge many reasons in support of standard dimensions of many of the parts of our astronomical and physical instruments, but it is not necessary, as the day has passed when we are satisfied with anything but interchangeable parts in modern machinery, therefore we should not be satisfied with anything less for our astronomical and physical instruments."

MR. W. BROOKS has left the observatory at Phelps, N.Y., which he established, for a first-class observatory, fully equipped with astronomical instruments, founded by Mr. William Smith at Geneva, N.Y. We congratulate Mr. Brooks on having more favourable conditions under which to carry on and extend his previous good work.

WE are glad to see from the 'Sidereal Messenger' for June that the Denver University Observatory, Colorado, is to have a new refractor of 20 inches aperture, for which the director has to thank the generosity of Mr. H. B. Chamberlin, of Denver. The instrument is to be mounted 5000 feet above the sea-level, or 800 feet higher than the Lick telescope.

THE Dearborn Observatory is being moved bodily to Evanston, 250 feet from Lake Michigan. Plans for the new building have been adopted, and its erection was to begin about the 1st of May ; but the 18½-inch equatoreal will not be mounted before January 1889.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 139. AUGUST. 1888.

Meeting of the Liverpool Astronomical Society.

SEVENTH ANNUAL MEETING, 1888, July 9.

J. E. GORE, F.R.A.S., in the Chair.

THE reading of the Annual Report, as will be seen from our *résumé* below, showed the Society to be steadily progressing; and the Balance-sheet proved the financial affairs to be very satisfactory. The total number of members now amounts to 641, being an increase of 216 since the last annual meeting. The *Journal* has been further enlarged to 316 pages of printed matter, and is become widely appreciated. The library is increasing, and is being very largely utilized by the members. "The financial state of the Society," to quote the Report, "continues satisfactory, and it is gratifying to notice that it has now proved self-supporting." The work done in the various sections during the past year showed a highly satisfactory result. In the *Solar Section*, although the work has been curtailed by the great decrease in solar activity, yet new members have joined this branch, and Messrs. Radmore and Brown have sent in useful reports and observations.

Thanks to Mr. Elger's energy the *Lunar Section* can show a year's work of great value. His papers and illustrations have been widely appreciated and admired.

In the *Planetary Section* many observations have been made on Jupiter, though under most unfavourable conditions; and a summary of results will be finished early next session. Several members, including Messrs. Smart, Kempthorne, and Brenan, have sent in some very interesting drawings of Mars. Mr. Elger continues his studies of Saturn, and has presented the Society with a valuable summary of recent observations, which appeared in the June number of its Journal.

In the *Variable-Star*, *Double-Star*, and *Cometary Sections* good work has been done owing to the valuable help given by Mr. Tarrant, the Rev. W. R. Waugh, and Mr. W. H. Maw.

By the report of the *Meteoric Section* it will be seen that much useful material has been accumulated; and Mr. Denning, supported by Mr. Booth, of Leeds, is to be congratulated on the carefulness and accuracy with which meteoric observations have been registered.

In the *Educational Section* the services of Mr. James Gill have been placed at the disposal of a large number of members, who have received useful instruction as to the best methods of observation and the management of their telescopes.

To the Pernambuco Branch belongs the credit of fostering a regard for Astronomy throughout Brazil. Meetings have been held twice in each month, and sixteen circulars of their proceedings published. A new branch of the Society has been formed in Douglas, Isle of Man, which already includes 27 members, and promises to increase rapidly. The nucleus of a library has been formed in each of these branches, and committees have been elected for the selection and purchase of scientific instruments for the use of members.

The past session has been marked by a novel experiment, which it is believed will exert a favourable influence amongst the brotherhood of astronomers, and cement the many friendships formed by correspondence &c. The Report refers to the publication of photographs of Associates and Members of the Society. The idea had its origin in the natural desire to become acquainted with the features of those whose work and writings are familiar. The great demand for the card of associates has warranted the issue of further photographs, the publication of which will be undertaken during the ensuing session.

It is under consideration to establish periodical examinations in Astronomy, and, with the cooperation of the Universities, to grant certificates of competency.

The Report concludes with the following paragraph:—

"The Council cannot allow this occasion to pass without referring with regret to the resignation of Mr. W. H. Davies as Secretary. The members will all realize that they are losing the services of one who, during a long period, has untiringly devoted himself to the Society's best interests. His zealous attention and promptitude in the conduct of affairs have been the means of its rapid development; and the position which the Society has now assumed in the scientific world is mainly due to his ready conception of the essential features of such an association, and to the policy of progress which he has consistently pursued in the face of many difficulties. With a deep regard for the Society's welfare he has cheerfully borne the heavy labours of the Secretaryship, and has shown a singleness of purpose and a disinterestedness which entitle him to our warm gratitude; and the Council feel that this expression will be echoed by every one of the Members."

After the reading of the Report and Balance-sheet, the ballot for the Officers and Council for the ensuing session took place, and the following were elected:—*President*, T. G. Elger, F.R.A.S.; *Vice-Presidents*, W. F. Denning, F.R.A.S., J. Gill, F.R.A.S., J. Hartnup, F.R.A.S., Herbert Sadler, F.R.A.S.; *Editor* and *Librarian*, I. H. Isaacs; *Secretary*, W. E. Rowlands; *Treasurer*, W. H. Davies, F.R.A.S.; *Council*, T. W. Backhouse, F.R.A.S., Miss E. Brown, T. W. Clarke, J. L. Coxon, W. H. Davies, Jun., J. E. Gore, F.R.A.S., George Higgs, Major E. E. Markwick, F.R.A.S., W. H. S. Monck, M.A., F.R.A.S., Walter Sang, K. J. Tarrant, and Major H. Watson, F.R.A.S.

While the scrutineers were engaged in looking over the balloting-papers, a short conversational discussion took place on Mr. Lockyer's meteoric theory of comets, in which Messrs. Sadler, Davies, and Gore took part. Some general conversation followed with reference to members' instruments.

On the motion of Mr. Herbert Sadler, seconded and supported by Messrs. Isaacs, Rowlands, and Clarke, a special vote of thanks was unanimously and warmly accorded to Mr. W. H. Davies for his services to the Society since its foundation.

Votes of thanks were then accorded to the retiring Officers and Council, to the Scrutineers of the Ballot, and to the presiding Chairman; and the meeting broke up after a pleasant and most enjoyable evening.

Meeting of the Astronomical Society of France.

1888, June 6.

M. C. Flammarion, *President*, in the Chair.

M. Flammarion read a paper on the Solar Eclipses of the 19th century, in which he showed the discordance between Oppolzer's charts and the results of observation. In answer to a remark of M. Oppert, he said he should not advise historians to reply upon these charts in their investigations.

M. Trouvelot presented a series of celestial photographs offered by Prof. Pickering, Director of the Harvard College Observatory. The straight trails of nebulous matter shown in the recent photographs of the Brothers Henry are also visible in Prof. Pickering's.

Thanks were voted to Prof. Pickering, who was unanimously named honorary member of the Society, on the proposition of M. Trouvelot and Colonel Laussedat.

Colonel Laussedat read a paper on a graphical method of calculating solar eclipses, two or three times more rapid than the usual numerical calculation.

M. Gaudibert sent a drawing of the lunar crater Flammarion. A rill extends through the northern part and crosses Reaumur, being interrupted by hills on the west of Flammarion.

M. Vallet sent drawings of various features of the Moon made with a small instrument. M. Petit, observations on the colour of Mars and Jupiter. M. Rengel, configuration of Jupiter, his satellites, and β Scorpii. MM. Cornillon and Bruguière, sketches of a large Sun-spot from May 11th to 23rd. M. Schmoll said that the spot was just on the limit of visibility to the naked eye from May 16th to 18th; he had also observed the lunar crescent on May 12, forty-two hours and a half after the new Moon, its breadth being from 30″ to 35″.

[The above was not received in time for our last number.—Eds.]

Meteor Notes.

On the evenings of July 8, 11, 12, and 13, I watched the sky during periods amounting on the whole to 10¼ hours, and 77 meteors were observed. On July 8 a well-defined shower of streak-leaving meteors was seen from a radiant at 3° +49°. This probably represents very early members of the Perseid shower. In 1885, on July 13, I recorded some similar meteors from the point 11° +48°, and in 1877, at about the same epoch, a radiant was found at 6° +53°. There is very strong evidence for believing that these several displays (together with that seen in 1887, July 18, at 19° +51°, 'Observatory,' no. 127, p. 313) have their origin in the great Perseid shower. The position of the radiants above mentioned, though far west of the radiant of the Perseids on Aug. 10, does not negative the assumption of their identity, because observations here in recent years have proved that the diverging centre of the Perseids rapidly shifts its place amongst the stars and is visible during a long interval. In confirmation of this early display I may mention that Mr. D. Booth, at Leeds, has, during the second week of July 1887 and 1888, registered several meteors suspiciously like the Perseids from a radiant which he fixes at 359° +53°. This is undoubtedly the same shower as those observed at Bristol, in the region S. of Cassiopeia; and many further observations are required in July, with the object of thoroughly sifting these successive displays of Cassiopeids and Perseids.

On July 8 I also noticed a few small, short meteors from a centre at about 310° +79°. On July 11, 6 rather swift meteors fell from a radiant at 319° +22°; this is the same stream as determined by Lieut.-Col. Tupman in 1870, July 8, at 316° +22°, and is No. 42 of his Catalogue. Between July 11–13 I also noted some rather swift, conspicuous meteors, leaving streaks, from a centre at 28° +30° in Triangulum, but the radiant was not very exact. In 1878 at the end of July I saw meteors emanating from the point 28° +28°, but the accordance, though good as to place, loses much of its weight as there is a difference of 17 days in the epoch. This year I also derived a radiant at 290° +59° in Draco, which is evidently a recurrence of a shower seen here in 1885, July 9–13, at 290° +60°.

During my recent observations I noticed no fireballs, but 5 of the meteors seen were very conspicuous. On July 12 at $12^h\ 33^m$ a very slow, yellowish meteor fell from 147° +57° to 142° +52½°, and on the 13th at $12^h\ 41^m$ a meteor with a very thick train of sparks and extremely slow motion passed from 242° +50½° to 183° +52½°; it was certainly not brighter than 1st mag., though the nucleus appeared to have a sensible diameter. Mr. D. Booth also recorded a small fireball on July 10 at $10^h\ 35^m$, moving from 347° +29° to 347° +13½°, rather slowly and leaving a long thin streak or train. W. F. DENNING.

Bristol, 1888, July 19.

Selenographical Notes.

DAMOISEAU.—From the western flank of the great walled-plain Grimaldi the country falls gradually to the level of the surface of the Mare Procellarum, the contour of the coast being for the most part well distinguished by an undulating line of demarcation. On this gentle incline stands the ring-plain Damoiseau which, though only some 23 miles in diameter, is, from the curious complexity of its immediate surroundings, and the peculiarity and somewhat doubtful character of its details, especially worthy of attention. It is a particularly difficult object to draw, as doubtless many observers have discovered, and, to a greater degree perhaps than any other lunar formation of its class, is affected by very slight changes in the angle of illumination. Unfortunately, all published representations of it, with the exception of Schmidt's, are on far too small a scale to be of any practical use to the student.

On 1887, December 27, 8^h to 9^h, it lay just within the morning terminator and was beautifully defined with powers 284 and 340 on my 8½-in. Calver reflector. At this early stage of sunrise its appearance is very noteworthy. So far as the southern half of the formation is concerned, it resembles a bright oval ring with a smaller bright ring of similar shape lying unsymmetrically within it, the space between the two rings being occupied on the west by a large, deep, circular depression, with a longitudinal depression north of it, both being filled with shadow; while nearly all the corresponding space between the walls on the east is in sunlight, and appears to be scarcely deeper than the floor of the inner ring. On the latter is a longitudinal marking with two craters at its southern end, well shown by Schmidt. But one of the most remarkable features in connection with Damoiseau is found on the north. Here the southern side of what resembles a wide smooth bank, sloping down towards the Mare, cuts athwart the rings and destroys their continuity. This bank is not shown in any of the maps, though I have drawn and seen it satisfactorily on many occasions, and have recently received a sketch of it, made Jan. 26, 1888, $10^h\ 15^m$, from Mr. G. T. Davis, of Theale, near Reading, in which a good-sized crater is shown near its southern edge. What seem to be the remains of another formation, scarcely inferior in size to Damoiseau, are represented by the brilliant curved strip of wall (concave towards the north-west) on the west, and a low ridge, completing the ring, which is very distinct under oblique light. The second depression mentioned in connection with the west wall of Damoiseau runs up to a spur or projection from the east side of this curved strip. There is a small crater at the south-western end of the latter which is very brilliant at sunrise, but at a later stage (when the long. of morning terminator is about 68°) seems to be without a border on the west—a noteworthy circumstance when taken in connection with the apparent destruction of a portion of the ring with which it is associated.

Two rills were prominent objects on December 27, on the south of Damoiseau, one answering to Neison's η, and another, closely parallel to it, still farther south. The first was traced from the irregularly-shaped depression Damoiseau B, down the slope in the form of a crater-rill, and ultimately, as a more delicate object, on the surface of the Mare to the east side of a mound, lying about midway between B and Damoiseau *e*. Schmidt seems to have followed it much further in a northerly direction, and shows it as associated with three prominent craters immediately east of *e*, only one of which I have been able to identify.

The incomplete ring, represented by the curved strip, both the rings of Damoiseau, and what Neison calls the "valley plain" (*a*) adjoining the latter on the south, seem to affect the linear arrangement, so frequently remarked in other lunar regions—their centres being roughly in a line, ranging, in this case, from N.N.W. to S.S.E.

On 1888, May 23, 9^{h} to 10^{h}, all the features referred to were again seen, with the exception of the rills, but less favourably, owing to variable atmospheric conditions.

Kempston, Beds, 1888, July 19. THOMAS GWYN ELGER.

Galileo Galilei and his Condemnation.

SOME points in the memorable trial of this great pioneer of scientific astronomy before the Inquisition at Rome in 1633 are discussed in a paper by Mr. Sedley Taylor which was communicated to the R.A.S., and printed in the 'Monthly Notices' for March 1878 (vol. xxxviii. p. 256). A short time after the publication of this paper some additional investigations were made into the subject; but as the results of these have never, I believe, appeared in any astronomical periodical, it occurred to me that the remarks here offered might be of interest to your readers, particularly as a suggestion made by Mr. Sedley Taylor is evidently untenable, and it does not now appear likely that any farther light can ever be thrown upon the matter.

The great fundamental fact with regard to the famous trial is that Galileo, being (in consequence of the publication in 1632 of his 'Dialogo sopra i due Massimi Sistemi del Mondo') "vehemently suspected" of heresy in accepting as true the Copernican doctrine of the revolution of the Earth round the Sun, was condemned to recant and abjure this belief, pronounced by the Inquisition to be false and heretical. Other penalties were laid upon the unfortunate philosopher, respecting which it is well known that much exaggeration has found its way into popular books. Into this I do not propose now to enter, merely remarking that the restrictions placed upon his personal liberty so long after the trial, as when Milton visited him in 1638, were such that the poet could speak of him even then as "the prisoner of the Inquisition." But my present purpose is merely to discuss a contested point with regard to the manner in which the condemnation itself was brought about.

In the year 1616 Galileo had been inhibited through Cardinal Bellarmine from teaching the motion of the Earth as a fact or otherwise than as a working hypothesis. In consequence of the publication of the above-mentioned Dialogues in 1632, he was summoned to Rome in the autumn of that year and arraigned before the Inquisition in April 1633. During his examination a document was produced, which was stated to have been issued on the 26th of February, 1616; in this he was further commanded not *in any way* to hold, teach, or defend that doctrine. Carl von Gebler[*] published in 1876 his 'Galileo Galilei und die römische Curie,' in which he pointed out the great improbability of the genuineness of this document. It was objected to him, however, by Prof. Berti ('Il Processo originale di Galileo Galilei,' 1876) and M. de l'Epinois ('Les Pièces du Procès de Galilée, 1877) that he had not seen the original protocols of the trial[†]. In consequence of this, Gebler, anxious for the establishment of historic truth in the matter, undertook (notwithstanding his failing health) a journey to Rome in the month of May 1877, and there made a careful copy of the Acts of the trial, which he published at Stuttgart in the following autumn, intending afterwards to incorporate it, with some necessary alterations, in a second edition of his book. This, however, he did not live to do; but an excellent English[‡] translation of it was published by Mrs. Sturge in 1879; and whilst it was in preparation she received a letter from the author permitting her to give in an appendix, not only a few of the more important documents, but the conclusions to which he had been led by his researches at Rome, modifying to some extent his former views. One result (which seems clearly proved *) is that the suspicious document was *not* a forgery of *later date*, but "indisputably originated in 1616." When, however, it is stated that this was not a forgery made after that date for the purpose of convicting Galileo of breaking his word and disobeying the injunction laid upon him and accepted by him, it by no means follows that it was a genuine document really communicated to him in that year and afterwards binding upon him. It is well known that Galileo, at his first examination before the Inquisition on the 12th of April, 1633, denied all knowledge of any such document, and maintained that the only prohibition he had received in 1616 had been that communicated to him by Cardinal Bellarmine, which allowed him to treat the Copernican theory hypothetically; and, as Gebler and Mr. Sedley Taylor point out, it is exceedingly unlikely that in doing so he was making a false deposition, for in that case he must have seen from the Inquisitor's questions that he had the protocol of the 26th of February before him. It is almost incredible that its existence should have entirely escaped Galileo's memory, had it been really issued to him. Moreover, had this been the case, we know, from the methods of procedure followed by the Inquisition, that it would have borne the signature of the accused and of the notary and witnesses thereto, with which it would have been easy to confront his denial. The existing document therefore, although doubtless composed at the time it professes to have been, in 1616, is in all probability not a statement (all due attestation of which is wanting in it) of what actually took place at that time, but a misrepresentation thereof. In order to omit no possible precaution in his endeavours to discover the whole truth, Gebler, when at Rome in 1877, addressed a letter to the then Secretary of State, Cardinal Simeoni, in which, after giving a short account of the investigations respecting Galileo's trial, he remarked that though the suspicion that the note of the 26th of February, 1616, might be a subsequent falsification was no longer tenable, yet it seemed very doubtful whether it contained a correct account of the proceedings at that time, and that the point could only be cleared up if either the original protocol or some confirmatory notice were discovered in the archives of the Inquisition. The Cardinal's answer, dated July 20, 1877, is as follows:—

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[*] It will be remembered that the death of this persevering student of the life of Galileo took place in 1878, the same year in which Mr. Sedley Taylor's paper appeared.

[†] These had been brought back to Rome from Paris in 1848; and some garbled extracts from them were published by Marini in 1850.

[‡] An Italian translation was also published at the same time.

"In seguito della richiesta fattasi da V.S. Illm̃a di avere dei documenti relativi a Galileo, mi recai a premura di commetterne le opportune indagini. Praticatesi le più diligente ricerche, vengo informato non esistere affatto negli Archivi i documenti che si desideravano."

Thus is it established that no other document is extant relating to the proceedings of the 26th of February, 1616, than the well-known annotation, of which the accuracy, as we have seen, is open to considerable suspicion. The production at the trial in 1633 of a legal protocol of the kind would have cleared up the whole affair and annihilated Galileo's defence. As no document of this kind was produced, nor any further inquiry made regarding his ignorance of the absolute prohibition, we may, it seems to me, fairly conclude with Gebler, "that in 1633 no other document existed about the Act of the 26th of February, 1616, than this note without signature" or other testimony to its accuracy, which may very well have been added to in the way suggested by Wohlwill and referred to by Mr. Sedley Taylor.

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* See Appendix III. of English translation.

Yet such were the means adopted to bring about the condemnation of the aged astronomer, and for forcing upon him the famous and pitiable abjuration of which Gebler* well says:—

"Wahrlich es bedurfte nicht mehr der materiallen Folterung, die geistige hatte genug, übergenug, geleistet."

Mr. Sedley Taylor's paper consists principally of two points—arguments similar to those used by Gebler in 1876 against the genuineness of the note said to have been issued in 1616; and suggestions that further examination should be instituted by the authorities of the Vatican into this question, and a more complete publication of the Acts of the Inquisition be made. Both these points seem, however, to have been settled by Gebler's expedition to Rome in 1877. The Acts have been now inspected by several persons; the document of Feb. 26, 1616, is not a later falsification, though it may very possibly contain an erroneous account of some of the transactions which occurred at that time; and it is not likely that any other documents are now in existence which can throw further light upon this matter.

One other point is perhaps not undeserving of notice. It is very remarkable that in the unreasoning efforts to condemn Galileo in every way possible, one particular count in the indictment against him is actually sound in itself, although an erroneous conclusion is connected with it in its expression. That count condemns him for teaching that the tides of the ocean prove the rotation of the Earth, denied to have any real existence. In the submission which he was induced to make at his second examination before the Inquisition, he admitted that he had attributed too much weight to this argument and that it might even be capable of refutation. Perhaps we shall not be far wrong if we look upon the submission as partly the result of reflection that he had been quibbling in maintaining that he only used the Copernican theory in his Dialogues as a working hypothesis, and not as actually true. The fact is that Galileo, not accepting the acts of the Inquisition as representing the Church, clung to the hope that the latter would ultimately officially sanction the true theory. This, indeed, was at last done; but not until a period long subsequent to the lifetime of Galileo, whose memory, notwithstanding his weakness, is dear to all astronomers. W. T. Lynn.

Blackheath, 1888, April 19.

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* "Auf den Spuren Galilei's," one of the essays in the second volume of his 'Nachklänge,' published by his son, W. von Gebler, in 1880.

M. Houzeau.

La science astronomique vient de faire une très grand perte : J. C. Houzeau, le successeur d'Ad. Quetelet à l'Observatoire royale de Bruxelles, est mort le 12 juillet dernier, à l'âge de 68 ans.

Il naquit à Mons (Hainaut) le 7 octobre, 1820, et fit ses études en partié au Collége de cette ville, en partie à l'Université de Bruxelles.

En 1846, il fut attaché en qualité d'aide à l'observatoire royal, qu'il quitta en 1849 pour des motifs d'ordre politique. Il voyagea, et fréquenta beaucoup les bibliothèques de Paris, de Londres et d'Allemagne : c'est là l'origine de sa profonde érudition astronomique.

Le Général Nerenburger, le plus distingué des géodésiens belges, l'attacha au service de la carte de 1854 à 1857, pour la mesure de la base et la détermination astronomique des coordonnées des sommets de 1er ordre du réseau.

Cette tâche achevée, Houzeau alla s'établir en Amérique, où il mena une existence peu propice à l'étude, successivement publiciste, géomètre-arpenteur et planteur.

Il profita toutefois de son séjour aux tropiques pour classer par ordre de grandeur, d'après ses seules observations, les étoiles visibles à l'œil nu dans les deux hémisphères. Ce travail, qu'a été publié sous le titre d'*Uranométrie Générale*, a obtenu un prix quinquennal à l'Académie royale de Belgique.

Ad. Quetelet, tout absorbé par ses beaux travaux sur la statistique, la météorologie et la physique du globe, sciences au développement desquelles il contribua activement avec le puissant concours de Gauss et de Humboldt, avait un peu négligé les services astronomiques, et n'avait pas laissé prendre assez d'initiative à son fils Ernest, observateur très consciencieux.

A sa mort, survenue en 1874, le Gouvernement commença par nommer une commission chargée de la surveillance de l'Observatoire.

La nomination du successeur d'Ad. Quetelet se fit longtemps attendre ; les savants les plus distingués du pays conseillèrent celle de Houzeau, que le Ministère était peu disposé à proposer, à cause, des opinions avancées qu'il avait conservées. Un journal a affirmé que cette nomination se fit grâce à l'intervention du Roi lui même, et j'ai des raisons de croire qu'il en a été ainsi.

A peine arrivé en Belgique, en 1876, Houzeau réorganisa entièrement l'observatoire, avec le concours du Gouvernement, qui ne lui marchanda pas les crédits nécessaires.

Le personnel, qui se réduisait à MM. Ern. Quetelet, C. Hooreman, A. Lancaster et L. Estourgies, fut considérablement augmenté et comprit bientôt 5 astronomes, astronomes-adjoints et assistants-astronomes, 5 météorologistes et assistants-météorologistes, 2 calculateurs, 1 lithographe, etc.

Les services astronomiques prirent une grande extension.

L'Observatoire acquit le grand équatorial de $0^{m}\cdot32$ (objectif de Merz, monté par Cooke), le cercle méridien de Repsold, et les appareils nécessaire à l'analyse spectrale des astres.

Le *Bulletin Météorologique* quotidien parut bientôt ; le météorographe universel de Van Rysselberghe fut définitivement installé à Bruxelles et dans plusieurs stations météorologiques du pays.

L'acquisition des nouveaux appareils et l'extension des services rendaient absolument indispensable l'agrandissement de l'observatoire, et, par conséquent, son transfert en dehors de l'agglomération bruxelloise.

Houzeau s'occupa très activement de cette question capitale. Les plans furent dressés, les crédits votés, le terrain acquis sur un plateau situé entre le bois de la Cambre et Uccle. Alors commencèrent les difficultés qui ont probablement amené la retraite de Houzeau.

En 1882, il organisa deux expéditions pour l'observation du passage de Vénus, au moyen de l'héliomètre spécial qu'il avait inventé dans ce but. Lui-même dirigea celle du Texas et y fit les observations, confiant à M. Niesten la direction de celle du Chili. La parallaxe du Soleil, déduite des observations faites simultanément dans le deux stations, a été trouvée de $8'',907 \pm 0'',084$.

Rentré en Europe souffrant, il se fixa dans le midi de la France, et adressa au Gouvernement sa démission à la fin de 1883, sans avoir, malheureusement, fait exécuter les plans qu'il avait concus.

Possédant des connaissances encyclopédiques, ses travaux sont très nombreux et excessivement variés : ils embrassent la géographie physique, la géologie, la météorologie, la philosophie de la nature, l'astronomie.

Je ne m'attacherai ici qu'à ces derniers.

C'est dans les *Astronomische Nachrichten* qu'il débuta en 1843 et 1844 (Nos. 492 et 496), par des articles sur la lumière zodiacale et sur l'aberration ; ce dernier travail eut l'honneur d'une réponse de J. Herschel (No. 520).

En 1853, il proposait une méthode pour la détermination de la latitude, de la longitude, de l'heure et de l'azimuth au moyen des observations de passages d'étoiles fondamentales dans deux verticaux (*Mém. Acad. Belgique*, t. xxv.).

Il publia, en 1859, un mémoire sur la détermination du rayon vecteur d'une planète nouvelle (*Bull. Acad. Belgique*, 2^e^ sér. t. viii.) ; et, en 1862, une méthode pour mesurer la parallaxe horizontale des astres (*ib.* t. xiii.), méthode sur laquelle il est revenu en 1871 (*ib.* t. xxxii.), pour proposer l'héliomètre de son invention.

Les astronomes connaissent cet héliomètre, au moyen duquel on a pu effectuer au Chili 606 pointés pendant le passage de Vénus sur le Soleil ; au Texas, les nuages ont empêché l'observation pendant une partie notable du phénomène.

En 1865, Houzeau a donné une méthode qui permet d'étudier à la fois les mouvements propres d'étoiles appartenant à deux groupes distincts, en renversant, par réflexion, l'un des groupes sur l'image directe de l'autre (*Bull. Acad. Belgique*, 2^{e} sér. t. xx.). Mais cette méthode n'a pas été mise à exécution.

Une note très intéressante (1873) est celle (*Bull. Acad. Belgique*, 2^{e} sér. t. xxxvi.), dans laquelle il déduit avec une exactitude, qu'on ne pouvait pas espérer de ce procédé, la direction du mouvement du système solaire dans l'espace de la composante commune à toutes les trajectoires des comètes qui pénètrent dans ce système.

Citons enfin ses belles observations sur la lumière zodiacale, qui ont paru dans son Résumé de quelques observations astronomiques et météorologiques faites dans la zône surtempérée et entre les tropiques (*Mém. in* 8° *de l'Acad. de Belgique*, t. xxv.).

Ses derniers travaux et les plus importants, travaux qui seront toujours consultés avec beaucoup de fruit dans tous les observatoires, et qui le mettent certainement au premier rang des bibliographes scientifiques contemporains, se rattachent tous à la grande œuvre qu'il avait entreprise et qu'il n'a pu achever entièrement: la *Bibliographie générale de l'astronomie.*

Déjà, dans les *Annales* de l'Observatoire de Bruxelles, il en avait fait paraître la partie la plus pratiquement utile, sous le titre de: *Constantes de l'Astronomie.* Plus tard, il étendit le cadre de cet ouvrage et publia son *Vade Mecum d'Astronome* (1882).

Le tome ii. et la 1$^{\text{ère}}$ partie du tome 1$^{\text{er}}$ de la *Bibliographie générale de l'astronomie* ont paru avec la collaboration de M. Lancaster; la 2$^{\text{ème}}$ partie du tome 1$^{\text{er}}$ était en cours d'impression lorsque Houzeau fut attient de la maladie qui l'emporta.

Jusque dans ces derniers temps, il continua à y travailler avec ardeur, et surtout à donner à son collaborateur les instructions nécessaires pour l'achèvement complet du *monumentum perenne* qu'il laissera après lui.

Vers la fin du mois d'avril, seulement, il dut absolument renoncer à tout travail; il s'éteignit le 12 juillet. F. Folie.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Photographic Chart of the Heavens.

Gentlemen,—

If I had ever entertained any doubt that the Paris Astrophotographic Congress decided that a catalogue of the stars to the 11th magnitude should be made, I certainly would not have expended upon the question the time and thought which I have given to it.

When I wrote the memoir which has been so vehemently denounced by you, nothing was further from my thoughts than to propose in the 'Bulletin du Comité Permanent' the execution of

any work not sanctioned by the Congress, or to bring forward any new or "astounding proposal" of my own.

That these views with regard to the matter in question are shared by Admiral Mouchez and Mr. Knobel is evident from their letters which are published in your June number. I also know that the same impression was received by the vast majority of those who attended the Conference; and, in fact, until the appearance of your article, I did not imagine that any one questioned these views.

I regret that you have thought fit in your May article to employ a tone so far removed from that in which it is customary to conduct discussions in a scientific journal.

Your June article, however, explains that your intention was to raise a "loud protest," because "a scheme, which you contend has not been assented to, or even considered, by the members of the Astrophotographic Conference, has been quietly launched." I admit that a loud protest was necessary if such were the case; but such protests may be couched in language less contemptuous and more conducive to good feeling amongst astronomers than that which you have employed. Assuming from the very different tone of your June article that this will in future be the case, I accept your explanation; only I dispute the facts of the explanation, believing them to rest upon a misunderstanding, on your part, of what really took place at the Congress.

Of course such a discussion cannot have any influence on what I believe to be the unanimous views of the Permanent Committee; but it may be of value in the pages of the 'Observatory' in order that the whole subject may be better understood in England.

It would, however, lead me to exceed admissible limits of space were I to discuss in full the various points which you have raised on their merits.

The justification of your attack is that I have proposed a scheme which *was not even considered by the Congress*; and I shall have justified my action if I show that the question of the Catalogue was considered and adopted by Congress, was remitted by Congress to the Permanent Committee, and by that Committee remitted to me for report.

To these points therefore I shall confine the present letter.

Your argument, which endeavours to show that I, together with the President, Mr. Knobel, and a large majority of members of the Congress, have misinterpreted some of its resolutions, is founded on your own interpretation of the following ones:—

> "17. Outre les deux épreuves qui doivent donner toutes les étoiles de la Carte du Ciel jusqu'à la 14ᵉ grandeur, il sera fait une série d'épreuves à plus courtes durées d'exposition pour assurer une plus grande précision dans la mesure micrométrique des étoiles de repère et rendre possible la construction d'un Catalogue.
>
> "18. Les clichés supplémentaires, destinés à la construction

du catalogue, contiendront toutes les étoiles jusqu'à la 11^e; grandeur approximativement. La comité d'exécution réglera le procédé à suivre pour s'assurer que cette condition est remplie.

"19. Chaque plaque photographique, destinée à la formation du Catalogue, sera accompagnée de toutes les données nécessaires pour obtenir son orientation et la valeur de son échelle; autant que possible, ces données se trouveront inscrites sur la plaque elle-même.

"Chaque plaque de cette espèce portera copie, bien centrée, d'un système de repères dérivés d'un réseau, et destinés à eliminer les erreurs qui pourront être produites par une déformation ultérieure de la couche sensible.

"Les autres détails relatifs au contenu supplémentaire des plaques seront fixés par le Comité d'exécution."

You supply the following interpretation of the concluding words of Resolution 17—viz. they merely mean that "the plates should be so taken as to be capable of accurate measurement, and this is decided by the specification that they shall render possible the construction of a catalogue."

This does not appear to me a very obvious or even a very plausible explanation, as the words "et rendre possible la construction d'un catalogue," under such an explanation, are absolutely useless and redundant; the full meaning of your interpretation being completely conveyed by the immediately preceding words "pour assurer une plus grande précision dans la mesure micrométrique des étoiles de repère."

I prefer to seek the true meaning of the resolution by following the discussion through which it was approached on pp. 65–67 of the Procès-Verbaux.

The mode of discussion followed at the Conference was not one to which we are accustomed in England. With us the President or Chairman generally takes no part in moving or seconding, but submits, in their proper order for voting, motions and amendments which have been moved and seconded by others. At the Paris Conference, and, I believe, at similar meetings elsewhere on the Continent, the President introduces the subject of debate, listens to the remarks and motions of the various speakers, and endeavours to formulate a motion which shall unite the views of all, or a majority of the meeting; if he fails to do so, amendments are proposed and voted upon as against the President's motion. Sometimes the first attempt on the part of the President to formulate such a motion calls forth further discussion without a direct amendment from any member; and then, without putting his original motion to the meeting, the President formulates a modified motion, which meets more general acceptance. Now this was precisely what occurred in the case of the motion in question.

At the second *séance* of the Astronomical Section on April 21, the President, Dr. Auwers, introduced the discussion by remarking that "it is therefore necessary to fix what magnitudes of stars should be reproduced on the photographs destined for exact measurement" (p. 66).

The first speakers in the debate which followed pointed out that on plates of short exposure, such as on the first day's discussion had been suggested to be made in addition to those of longer exposure, a measurement of the coordinates of stars could be made with high precision. That it was impossible to imagine that all the stars to the 14th magnitude should be catalogued, but that those of, say, the 11th or 12th magnitude might be so. That the *desiderata* of astronomers in this respect should be discussed and formulated, and that the details of the execution of the work should be afterwards discussed by a committee consisting of a limited number of the members.

The President therefore submitted the following tentative resolution:—

> "On fera des plaques spéciales destinées à la formation d'un Catalogue."

M. Cornu then rose to point out that on the preceding day a proposal still before the meeting had been made; and he proposed to combine this motion with the previous motion as to a catalogue. This motion is so important that it must be quoted, together with the explanatory remarks of the mover:—

"M. Cornu fait remarquer que la Comité a été réunit en vue des moyens d'exécution d'une Carte du Ciel jusqu'à la 14^{e} grandeur; ce point est acquis par un vote antérieur du Congrès tout entier. Il faut maintenant décider si, à ces cartes, sera joint un Catalogue: un Catalogue général serait impossible; il faut donc limiter la question et la restreindre à des étoiles du 10^{e} ou 11^{e} grandeur. M. Cornu appuie donc la proposition antérieure de M. Fizeau, et celle de M. Gill, d'obtenir une épreuve spéciale en vue du Catalogue et propose la resolution suivante:

> "En autre de la photographie destinée à la Carte du Ciel, il serait fait des épreuves à plus courte durée d'exposition en vue des mesures micrométriques destinées à la formation d'un Catalogue."

After a prolonged general discussion, a great part of which is not reported and only the names of the speakers are given, M. Cornu's motion, in the following slightly modified form, was put to the Meeting, and adopted with only one dissentient vote (probably that of Mr. Knobel, to judge from his frank and manly letter which appears in your June number):—

> "Outre l'épreuve qui doit donner les étoiles jusqu'à la 14^{e} grandeur, il sera fait une série d'épreuves à plus courtes durées d'exposition, pour assurer une plus grande précision dans la mesure micrométrique des étoiles de repère et rendre possible la construction d'un Catalogue."

I have a very vivid recollection of that discussion. The phrase "pour assurer une plus grande précision dans la mesure micrométrique des étoiles de repère" was introduced to meet the views of a small minority who wished to limit the general catalogue to "les étoiles de repère," and the words "rendre possible la construction d'un Catalogue" to meet the views of a very small minority who were opposed to the principle of a Catalogue at all, and who apparently intended to oppose the principle of the Catalogue at a later stage.

The following morning (April 22) Dr. Auwers, as President, submitted to the Astronomical Section the following proposal:—

> "Le clichés supplémentaires destinés à la construction du Catalogue contiendront toutes les étoiles jusqu'à la 11ᵉ grandeur inclusivement. Le Comité d'exécution réglera les procédés à suivre pour s'assurer que cette condition est remplie."

Note here that the question now is no longer one *d'un Catalogue* but *du Catalogue*—not a question of what stars *a* possible catalogue is to contain, but what *the* catalogue shall contain.

That there may be no mistake about this the President adds the following specific definition and explanation of his motion (p. 69):—

> "On a, en effet, un Catalogue du Ciel boréal qui renferme les étoiles jusqu'à la 9ᵉ grandeur, et, avant la fin du siècle, un Catalogue analogue existera sans doute pour le Ciel austral; il est donc nécessaire d'aller plus loin, dans l'ordre des étoiles à cataloguer, afin d'accroître nos connaissances sur la distribution des étoiles et les mouvements propres. Pour que ce travail ait une importance suffisante, il parait convenable de pousser jusqu'à la 11ᵉ grandeur, ce qui decuplera environ le nombre des étoiles cataloguées; il ne parait pas impossible de cataloguer un million d'étoiles."

In the discussion which followed one member expresses the wish that the Catalogue should not be extended beyond the 10th magnitude, on the ground that the images of the stars would be sharper and the number of stars be sufficient for "le raccordement des clichés."

Another objects to the principle of a general Catalogue, and insists that the object should not be a Catalogue to a given order of magnitude, but should be limited to the stars necessary as "étoiles de repère." One member desired to push the order of magnitude higher in certain regions of the sky. Mr. Common, who may perhaps have had some difficulty in following very closely the course of the discussion, objected to the President's motion, on the ground of the number of photographic plates that would be required, apparently failing to see that the number of plates had been decided upon the previous day.

The other speakers insist on the necessity for the Catalogue containing the stars to the 11th order of magnitude, and the motion of the President is carried by a large majority "(environ 25 voix contre 6)."

The resolutions of the Astronomical section were brought up for discussion by the General Congress on the 23rd April, and were brought forward for discussion *en bloc* by the President, M. Struve.

The first speaker proposed the following modification on the second resolution (afterwards No. 18 of the general resolutions):—

> In lieu of "la 11ᵉ grandeur inclusivement"—"jusqu'à une grandeur qui ne dépassera pas la onzième, et qui sera fixée ultérieurement par le Comité permanent," further demanding "*qu'on ne parle pas uniquement du Catalogue; on pourrait dire que les clichés devront être pris de manière à permettre la possibilité de former un Catalogue*; il faut avant tout avoir la Carte photographique et les positions des étoiles de repère."

In order to combat that portion of the amendment, as defined by the remarks of the mover, which I have underlined, M. Bailland "voit des inconvénients à renvoyer au Comité permanent la question de savoir si l'on fera un Catalogue ou si l'on n'en fera pas" (see page 23 *loc. cit.*).

Here was a clear and distinct issue as to whether the Catalogue was "to be or not to be"—the question of a definite as against an indefinite resolution.

After a long debate the mover of the amendment was asked by the President whether he would accept the original motion if the words "onzième grandeur" were replaced by the words "onzième grandeur approximativement."

This the mover declined to accept, and after some further discussion, on the demand of several members, resolution 17 was unanimously adopted. The amendment first moved was then rejected, and, finally, the original motion of the President with the amendment "onzième grandeur approximativement" was adopted by a large majority, 6 votes only being recorded against it. The meeting consisted of 50 members.

The Meeting further passed the following important resolutions relative to the Catalogue:—

> "22. Le choix des étoiles de repère est renvoyé au Comité d'exécution.
>
> "23. La question des méthodes de mesure et de la conversion des nombres obtenus en ascensions droites et en déclinaisons pour l'équinoxe de 1900 est renvoyée au Comité d'exécution. Le Comité s'occupera en premier lieu de l'étude et de l'emploi des instruments de mesures pouvant donner à volonté des coordonnées rectangulaires ou polaires et basés sur l'emploi simultané des échelles pour les grandes longueurs et de vis micrométriques à l'oculaire pour les subdivisions de l'échelle."

Thus not only is it decided that a Catalogue shall be made, as opposed to the definition of the amendment that "the plates shall be taken in such a manner as to permit the possibility of forming a Catalogue," but leaving details of execution and more precise definition to the permanent Committee, the Conference leaves the choice of the Standard Stars for each plate to the same Committee; it fixes the equinox of the Catalogue to be 1900; it remits to the permanent Committee the method of measurement and of the conversion of the numbers obtained into right ascension and declination for that equinox; it directs the Committee in the first place to discuss the best methods of measurement, specifying certain essentials in the instruments to be used for the purpose.

Such were the powers and instructions of the General Conference given to the permanent Committee.

Soon after my return to the Cape, pursuant to these powers and instructions, I received a letter from Admiral Mouchez, as President, requesting me, in the name of the permanent Committee, to write a memoir on the method of mounting the plates, and on the whole method of working generally. The result was the memoir which treats of what you insist on calling "Dr. Gill's Catalogue," which you feel bound to oppose "vehemently" as an "astounding proposal"—"a scheme which has not been assented to *or even considered* by the members of the Astrophotographic Conference," &c.

I leave your readers to judge of the justice or otherwise of your remarks.

Yours faithfully,

DAVID GILL.

Royal Observatory,
Cape of Good Hope, 1888, July 3.

Intersecting Rainbows.

GENTLEMEN,—

From a commonplace book of mine, when an undergraduate at Cambridge, I extract the following, dated forty-four years ago:—

"A friend of mine, Dr. Black, of Manchester, sailing up the Clyde with a party of scientific men to be present at the proceedings of the British Association, saw a very curious double rainbow. The appearance was that of two primary eccentric bows, one smaller, and the other, which was the fainter of the two, greater than a semicircle." [Here follows the explanation, with two figures of mine, in one of which S'T' is the line joining the Sun's centre and the point on the water of the reflection of the Sun's centre; *s*, the centre of the Sun's image. The bows are drawn, of course, intersecting.] "Dr. B. was the first, I believe, who attempted to explain this phenomenon. His [correct] account of the matter, however, was not at first satisfactory to [let us hope *all* of] the rest of the company. But Nature herself interfered experimentally, as it were, to set it right. A dark curtain of clouds happened to be floating at the time in such a direction as gradually to drop over the upper or direct rays which caused the lower bow, and to cut off, first the highest, and then, piece by piece, every successive portion of the lower bow, *leaving the upper one entire*: they had not as yet extended low enough to interfere with the rays in the direction S'T', by the reflected agency of which the upper bow was produced, as was clear to the party on looking at the water behind them, in which they saw the image of the Sun at *s* unimpaired.

"This is one of the most beautiful phenomena, upon the whole, that I ever heard of, and it is also a rare instance of spurious bows.

"S. Joh. Coll., Jan. 1844."

Yours faithfully,
EDWARD ALLEN.

Castlechurch Vicarage,
1888, July 20.

OBSERVATORIES.

PARIS.—We have received Admiral Mouchez's annual report for the year 1887. He remarks that two events of importance occurred during the year—the Astrophotographic Conference assembled in April, at Paris, and the first two volumes of the Paris Catalogue were distributed in December. It is not necessary to say anything further here about either of these matters. As regards the regular work of the Observatory, the approaching completion of the observations for the Catalogue has somewhat modified the ordinary plan of operations. The few remaining stars are to be observed exclusively with the great transit-circle; and at the same time the fundamental stars are to be carefully reobserved, a work for which there has hitherto not been sufficient leisure. The other meridian instruments are being used for separate investigations, notably that of the latitude of the Observatory. One result for the latitude given in the present report depends on observations of Polaris above and below pole in the month of June; but a caution is justly added as to the influence of refraction; indeed, we should be inclined to consider such a result as liable to a considerable correction depending on the time of year unless the atmospheric conditions at Paris are abnormal. A completer investigation is being undertaken by M. Renan. In all, over 16,000 meridian observations have been made with 4 instruments by 14 observers, which is a very creditable performance. With the equatoreals 65 observations of 5 comets and 11 of 3 small planets have been made; and 720 measures of the distances of lunar craters from the limb, with a view to determining the libration; and a considerable amount of work has been done in connection with M. Loewy's new methods for determining aberration and refraction. Besides this, the Brothers Henry have continued to work hard at photography, and have taken 74 plates of different parts of the sky. An engraving of the Pleiades, compiled from three of their magnificent photographs, accompanies the report, and shows so much new nebulous matter that one is led to wonder whether it is not merely a question of exposure to discover that all space is more or less nebulous.

The regular meteorological and time signal observations have been continued, and there has been considerable activity among the various members of the staff in minor investigations.

PUBLICATIONS.

First Report of the Thunderstorm Committee of the Royal Meteorological Society.—During the summer of the year 1887 the Royal Meteorological Society prepared and distributed widely to photographers and photographic societies a circular asking for photographs of flashes of lightning, in response to which about 60 photographs were received, forming a very interesting collection. These photographs were exhibited at the March meeting of the Royal Meteorological Society and at other places; and have attracted a good deal of attention, having revealed to us in a striking manner the actual forms of lightning-flashes, which, in many respects, are different from what has been previously supposed. The Society has also organized a Thunderstorm Committee, with the view of making systematic inquiry into the phenomena of thunderstorms, although this first report deals only with the photographs of lightning above mentioned. The Committee consider that lightning-flashes may be grouped as follows:—1. Stream; 2. Sinuous; 3. Ramified; 4. Meandering; 5. Beaded or Chapletted; 6. Ribbon. Some peculiarities shown by the photographs require further consideration; and it is very desirable that all those who are able will endeavour to give assistance in this very interesting inquiry. The report contains excellent autotype reproductions of four specimens of lightning-flashes; and it is pointed out that this collection in no way gives support to the zigzag or forked form of flash so commonly seen in pictures. W. E.

O'Gyalla.—The ninth volume of the "Observations" contains those made in the year 1886, which have been published with commendable promptitude. In the Introduction Dr. Konkoly describes the instruments and the methods of determining their constants. Of the work done, by far the most original is the spectrum-photometry of 34 fixed stars, and the planets Mars, Jupiter, and Saturn. The intensity of the light at 7 different points of the spectrum was measured with a wedge photometer, and the results discussed both with reference to the character of the spectrum of stars of different types, and to the whole radiation energy of the individual stars. The difficulties of the measures are naturally so great that the separate results are in many cases not very accordant; but the number of observations is sufficient to justify some confidence in the final values obtained. The following table gives the number of mechanical units of radiation energy (milligrammillimetres per second per square centimetre) for a few stars:—

Type 1.		Type 2.		Type 3.	
α Canis Maj. .	4·68	α Aurigæ . .	1·43	α Scorpii . .	1·06
α Lyræ	1·96	α Boötis	1·36	α Orionis . .	0·72
α Aquilæ . .	1·91	α Cygni	1·24	α Tauri	0·65
α Leonis	1·12	γ Leonis	0·64		
α Geminorum	0·59	α Ursæ Maj.	0·62		
γ Cassiopeiæ	0·31	β Geminorum	0·61		

The examination in the same manner of the spectra of the planets gives some information as to the selective absorption of their atmospheres. It appears that the spectrum of Jupiter is about halfway between those of types I. and II., that of Saturn being nearer type I.; in that of Mars a large absorption of the violet rays is naturally found. Some nebulæ and comets and some special stars have also been examined either photometrically or with the spectroscope. There are numerous notes regarding the aspect of the Sun's surface on each day available for observation, and a table of sun-spot positions in the year 1886. Very little information is given as to how these positions were obtained, and what elements of reduction were used, which somewhat detracts from their value; but, as far as can be judged, they accord well with the positions obtained at Greenwich from measurements of photographs.

A large number of meteor observations made at O'Gyalla, Presburg, and Buda Pesth, with a list of radiants deduced by Mr. W. F. Denning, completes the volume. T. L.

REPORT ON THE CAPACITIES IN RESPECT OF LIGHT AND PHOTOGRAPHIC ACTION OF TWO SILVER-ON-GLASS MIRRORS OF DIFFERENT FOCAL LENGTHS. By the Rev. C. PRITCHARD, D.D., F.R.S., Savilian Professor of Astronomy, Oxford (Proceedings of the Royal Society, Vol. xliv. p. 168).—In this report Professor Pritchard gives a most interesting account of experiments on the comparative photographic capacities of two silver-on-glass mirrors of 15 in. aperture, one of 80 and the other of 120 inches focal length. These experiments were undertaken at the request of a Committee on Stellar Photography appointed by the Council of the Royal Society. The mirrors were made by With, and were provided by the generosity of Dr. Warren de la Rue. The points to which Prof. Pritchard chiefly directed attention in his examination of these mirrors were:—1. The general and comparative character of the stellar images obtained from the two mirrors; 2. The relative amounts of light reflected by each; 3. The relative tendency of the mirrors to distortion of stellar images at different parts of the field; 4. Their capacity to photograph faint stars with given exposure. In making these experiments many difficulties were encountered, as might have been expected in such an investigation; but these seem to have been most successfully overcome. The mirrors were mounted in turn on the tube of the large equatorial in the Oxford University observatory. In the experiments to determine the general character of the stellar images Prof. Pritchard used at first a plate 4 in. square which, with a circular carrier of 7½ in. diameter, was placed directly in front of the mirror in the principal focus; but finding that this size of plate produced malformation of the images, he replaced it by a smaller plate in a smaller carrier for the mirror of shorter focus, though he appears to have retained it for the longer. The conclusions at which Prof. Pritchard arrives are—(1) the general unsuitability of mirrors of short focal length, and (2) the impossibility of obtaining with mirrors a large field such as would be serviceable in charting the heavens by means of photography. He finds also that the images of bright stars impressed by the mirror of short focus have a greater tendency to spread themselves over the film than in the case of the longer-focus mirror; with the smaller stars this peculiarity is not so apparent. A comparison was made of the relative luminosity of the images of stars formed by the two mirrors by means of the wedge-photometer, stars being viewed respectively in the mirrors, the 12¼ in. Grubb refractor, and in the 4 in. finder attached to the latter. Two most important results were arrived at:—1. That the deterioration of the mirrors after nine months constant use and exposure is very slight. 2. A very large amount of light is reflected by these mirrors in comparison with that transmitted by the Grubb object-glass, viz.:—a mirror of 15 in. aperture affords an image as brilliant as that formed by an object-glass of the particular quality presented of 13·35 inches aperture. The more important investigation, of determining the angular extent of the apparently undistorted field, has been very carefully carried out, and Prof. Pritchard finds from his investigations that whilst the angular distance from the centre of the plate of the shorter-focus mirror at which the stars cease to be sufficiently circular is 40′, in the case of the longer-focus mirror this angular extent of measurable field extends beyond 51′.

Prof. Pritchard remarks, "This very perceptible superiority of the longer-focus mirror arises partly, no doubt, from its longer focus, and it may also be influenced by the intervention of the plate-holder. But be the causes what they may, the superiority of the longer focus is unquestionable in respect of extent of field."

With regard to the amount of distortion which exists at different distances from the centre of the plate, Prof. Pritchard gives the measurements of pairs of stars taken on different parts of the plate, and he finds that there is no perceptible or measurable distortion in the apparent distance of the pair of stars on a plate of 1° 55′ square in the case of the longer-focus mirror. With the shorter-focus mirror the linear discrepancies of the measured distance are the same as with the other; they therefore indicate larger angular discrepancies in the ratio of three to two, and he finds a corresponding increase in distortion. In considering the capacities of the two mirrors for photographing faint stars with exposures of given duration, he finds that the shorter-focus mirror has a slight advantage over that of longer focus.

Prof. Pritchard is to be congratulated on having done something towards defining the capabilities of the reflecting telescope for such work as charting the heavens by photography, and it is to be hoped that this is only the beginning of a series of such investigations, and that even the present experiments will be extended.

In reading the report we cannot help regretting that the shorter-focus mirror was not exclusively used, as by merely using a diaphragm on the face of the mirror of any given diameter any ratio of aperture to focal length could have been obtained. For instance, it would be most interesting to know if reducing the $\frac{15}{80}$ mirror to $\frac{10}{80}$ would have had the effect of extending the undistorted field beyond the 40′ obtained; this could be done without disturbing the adjustments of the plate; indeed, it seems hardly possible to come to any definite conclusions as to the effect of different ratios of aperture to focal length without first being sure that the other conditions are kept identical.

The comparison of silver-on-glass mirrors with metallic mirrors as regards their photographic action is another important point; in fact, the number of such points yet to be determined, and which naturally suggest themselves in reading this report, is enormous. Prof. Pritchard has unusual facilities for doing such work, and such results are extremely valuable at the present time, when gigantic schemes are being undertaken without adequate assurance that the right method is known.

NOTES.

THE TOTAL LUNAR ECLIPSE OF 1888, JANUARY 28.—Dr. Döllen has now collected the results from the various observatories which were favoured with fine weather on January 28, and finds that he has observations of 783 phenomena (viz. 396 disappearances and 387 reappearances at 55 different places), counting as one observation only more than one record of the same phenomenon at the same place. The places of observation are so favourably distributed that he considers there is ample material, if it is found amenable to treatment, for determining the place, diameter (with possible ellipticity), and parallax of the Moon; indeed for the last-mentioned element he has bases of 90° in latitude and 150° in longitude.

WE have received the following from Mr. Chambers:—

GENTLEMEN,—I am preparing for publication next year a new edition of my 'Descriptive Astronomy.' A book of this character must necessarily have many general defects, and contain a certain number of errors. Allow me to notify to the astronomical world through your columns that I am prepared to welcome suggestions and corrigenda from any and every quarter.

Yours faithfully,
Northfield Observatory, Eastbourne, Sussex, GEORGE F. CHAMBERS.
1888, August 10.

THE ORBITS OF METEORITES.—Prof. H. A. Newton, of Yale College, has carefully studied the evidence available for determining the "former orbits of those meteorites that are in our collections and that were seen to fall." Of these stone-falls there are three classes—(*a*) 116 falls for which we have statements as to the direction of the path through the air; (*b*) 94 falls of which we know the time of day; (*c*) 50 or more falls of which the history is too scanty to give the time of day. He is led to the following three propositions:—

1. The meteorites which we have in our cabinets, and which were seen to fall, were originally (as a class and with a very small number of exceptions) moving about the Sun in orbits that had inclinations less than 90°; that is, their motions were direct, not retrograde.

2. The reason why we have only this class of stones in our collections is not one wholly or even mainly dependent on the habits of men; nor on the times when men are out of doors; nor on the places where men live; nor on any other principle of selection acting at or after the arrival of the stones at the ground. Either the stones, which are moving in the solar system across the Earth's orbit, move in general in direct orbits, or else for some reason the stones which move in retrograde orbits do not in general come through the air to the ground in solid form.

3. The perihelion-distances of nearly all the orbits in which these stones moved were not less than 0·5 nor more than 1·0, the Earth's radius-vector being unity.

We are glad to learn that Sir Howard Grubb has at length obtained, from Messrs. Chance, what promises to be a satisfactory crown disk for the 28-in. refractor which he is constructing for the Royal Observatory, Greenwich. It only remains to remove one small bunch of veins, which does not appear a difficult matter.

Prof. Antonio Favaro, of the Royal University, Padua, being engaged on the supervision of a new and complete edition of the 'Works of Galileo,' would be very glad of any information respecting Galilean documents with which collectors of old manuscripts and others may be able to assist him.

Mr. Clement Wragge is doing good Meteorological Work in Queensland. Weather-charts on a large scale, embracing observations made at 72 stations in Australia, New Zealand, and Tasmania, are published every morning, and forecasts issued every evening.

We notice with great regret that Prof. Newcomb is seriously ill of spinal irritability. We sincerely hope that he will soon recover his health and vigour. A man of his ability and energy can ill be spared, even for a few weeks.

An astronomical observatory is to be established in connection with the Foreign College, which is expected to be opened at the end of this year, in Pekin.

The order of the Rising Sun has been conferred, by the Mikado of Japan, on Prof. Milne for his seismic investigations at Tokio.

Messrs. Sampson Low and Co. have in the press the 'Life and Correspondence of Abraham Sharp,' assistant to Flamsteed.

Minor Planet No. 278 has been named Paulina.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 140. SEPTEMBER. 1888.

The Photographic Chart of the Heavens.

Important as the question is which we raised in May last with regard to a proposed Catalogue of 2,000,000 stars, we fear that to some of our readers the resulting discussion may have already appeared somewhat inordinately lengthy; and we rejoice for their sake, as well as our own, that we can now anticipate a probable speedy conclusion to it so far as we are concerned. A most important contribution to this discussion appeared in our last number, in Dr. Gill's reply to our two articles of May and June. Dr. Gill carefully describes the manner in which the various proposals for a Catalogue were dealt with at the Conference; and it might at first sight appear that his letter calls for an equally careful and equally lengthy reply. Such a reply we claim, in all humility, to be able to produce, did it serve our purpose; but we prefer for many reasons to make a simple remark which may tend to throw a clearer light on the question at issue—the question whether a catalogue of all stars down to the 11th magnitude inclusive, about 2,000,000 in all, was definitely sanctioned by the Conference as part of the international scheme. We contend that such was not the case: Dr. Gill undertakes to prove that it was; and it takes an advocate of his ability and ingenuity more than five pages of print to adduce the evidence! Now we regard this as conclusive: such a Catalogue would be a sufficiently new departure to justify the demand that there should be *no possibility of mistake* about the authority for it. It may be that individual members of the Conference (as Dr. Gill himself, if we are not mistaken) were anxious for such a Catalogue, and went to the Conference hoping that it might be found possible to undertake this work. It is also pointed out by Dr. Gill himself that this was by no means the only project for a Catalogue before the Conference; to take a single instance, a Catalogue of stars to the tenth magnitude only was considered. It therefore behoved those present, and especially those who definitely advocated the Catalogue of 2,000,000 stars, to take every care that the matter should be brought to a clear and distinct issue if they intended subsequently to avail themselves of the resolutions to favour their project. Dr. Gill's careful and ingenious method of deducing his authority defeats its own end, by immediately suggesting that confusion which it is his endeavour to show did not exist. We see no reason, therefore, to yield one inch of the position we took up at first. The question will of course come in time before those whom it more immediately concerns; and it is perhaps unprofitable to discuss it further in these pages. We hope, however, that events will prove that it has not been useless to draw attention, as we have done, to the different interpretations of which certain resolutions passed by the Conference are apparently susceptible. EDITORS.

Regarding Sir William Herschel's Observations of Volcanoes in the Moon.

ON the 26th of April, 1787, Sir William Herschel communicated to the Royal Society of London a paper in which he gives an account of three volcanoes which he had seen in activity on the Moon, April 19 and 20, 1787; and he also speaks of another which was visible May 4th, 1783.

I have never been able to understand how Herschel, the keenest of observers, could have been deceived in this observation until the night of July 15th of this year, when I was looking at the Moon with the great telescope. At the southern extremity of the Alps, in the dark portion of the disc not far from the terminator, I saw an illumination of the crest of a high peak which was extraordinarily and incredibly bright. It was 40 to 50 seconds of arc in length, and about five seconds wide at its southern end, and ten to fifteen at its northern end, with marked curvature on its northern third. No part of this illumination seemed less bright than a first-magnitude star, and taken all together it was the brightest object I have ever seen in the sky. It was apparently ten times as bright as the neighbouring portions of the Moon's surface. Its yellow light was tinged in places with the purple due to the secondary spectrum of the objective; and viewed as a whole it presented the appearance of a vast conflagration—something quite foreign to the brilliant white of the rest of the Moon's surface.

It would have required no stretch of the imagination to have supposed it to be a tremendous eruption of a range of lunar volcanoes. I suppose that a corresponding view of an eruption of *Kilauea* would have been quite insignificant in comparison. Observations on this and the succeeding nights showed that it was in fact due to a specially brilliant and favourable illumination of a mountain ridge near the southern termination of the lunar Alps.

I have now no doubt that the observation of Sir William Herschel referred to similar appearances. The diameter of the volcanoes seen by him he estimated as twice that of Jupiter's third satellite. I judge that the brightness was far less than that of the appearance seen here, both from his description (Phil. Trans. 1787, p. 231), and from the fact that he was observing with a ten-foot reflector. The Moon was about two days old at his first observation of 1787 and at his observation of 1783.

I find that the very same region which was so extraordinarily brilliant on July 15th has been previously observed to present this exceptional appearance. On 1843, July 4th, at 9¼^{h}, Dr. Gerling of Marburg, observed a "splendid bright point, with a light like a fixed star, except that a trace of definite shape was still to be perceived. It was in the southern prolongation of the lunar Alps, and its distance from the crater *Autolycus* was estimated as equal to that of *Autolycus* from *Cassini.* On the following evening a small round isolated conical mountain was found in this spot, which presented no especial features" (Astronomische Nachrichten, no. 526, col. 356). I have no doubt that Dr. Gerling and myself saw the same object under similar conditions. The object viewed was "splendid" as seen by him with a 5-foot Fraunhofer refractor and a power of 54. The phenomenon as seen here was entirely unprecedented in my experience, especially as to its extraordinary brightness. I have now no doubt whatever that Sir William Herschel's observations of lunar volcanoes refer simply to apparitions of this kind. EDWARD S. HOLDEN.

Lick Observatory, 1888, July 19.

Selenographical Notes.

RILLS UNDER NORTHERN WALL OF PLATO.—The *glacis* of Plato includes objects of interest on every side, but at one point on the north, immediately east of the great dyke which is clearly associated with the "fault" that has dislocated the border, there are certain features in the form of clefts especially worthy of examination when the Sun's altitude on the site is about 9° or 10°. Without a sketch it is difficult to convey an intelligible notion of the position and arrangement of these curious objects, and unfortunately, so far as I know, they are not specially mentioned or drawn in any selenographical work. The late Mr. Birt is, I believe, the only observer who has noted the existence of clefts in this position. In the 'Intellectual Observer,' vol. xi. p. 273, there is a drawing of Plato by him, made 1863, Jan. 11, 13^{h} to 19^{h}, with the Hartwell equatoreal of 5·9-in. aperture, in which he shows two of the rills in question and describes them as lying east of a mountain range, which is evidently, from his sketch, the dyke just referred to. At 9^{h} on March 21, conditions were favourable for an examination of the locality and a power of 340 afforded me a fine view of the clefts. The longest and broadest member of the group forms (speaking roughly) a tangent to the ring at a point a little east of the peak *e*. It appears to cut through the dyke and to run obliquely down the slope, terminating suddenly on the east at a point ten miles or so from the wall. This is the rill shown in Birt's drawing as running from W.S.W. to E.N.E. A second rill, nearly as coarse, runs parallel to the last at a distance of four or five miles. Its west end abuts on the dyke and it extends somewhat further down the *glacis* than the first, and is less straight and more irregular in width. This object is not shown by Birt. These rills are crossed nearly at right angles by two others, the more westerly lying nearly parallel to the dyke, and the second (answering to Birt's second rill) commencing at the more northerly of the parallel clefts, crosses the other cleft discovered by Birt, and, following a somewhat undulating course, runs up to the break in the north-east wall immediately west of the supposed landslip. I believe that under higher powers and more perfect atmospheric conditions all these clefts would be seen to consist of rows of inosculating craters, and that the presence of other more delicate furrows would be revealed in connection with them, which were only imperfectly glimpsed on March 21. It is strange, seeing how distinct the four cross clefts were on this evening, that they have not been recorded by Mädler, Schmidt, Neison, or other selenographers, especially when it is remembered that Plato has perhaps been more frequently examined than any other lunar formation.

SCHROETER'S χ, NORTH OF PLATO.—The shallow ring so prominently represented by Schroeter in t. xxi., xxiii. Sel. Top. Frag., has so seldom, if ever, been satisfactorily made out, that it is much to be wished that observers would try to obtain good sketches of it. I have never succeeded in detecting any object comparable to this ring in definiteness of form. There are curved ridges and a small deep crater near the site, which possibly under certain conditions of illumination may assume the appearance of a large shallow depression such as he represents. A little north of an imaginary line joining Plato A and the deep crater just mentioned, there is a curious incomplete ring of a horse-shoe form abutting on an oblong-shaped mountain, which I believe has frequently been mistaken for Schroeter's χ.

NEW RILL NEAR CAUCHY.—In a letter recently received from Mr. Victor Nielsen of Copenhagen, he informs me that he has detected with his 6½-in. refractor a delicate rill north-east of Cauchy. From an enclosed sketch this object would appear to run from a break in rill δ (Neison, Map I.) in a northerly direction, and to join the winding rill north of δ, described in the 'Observatory,' Oct. 1887, p. 354. He draws δ with two branches, which leave the main rill at a point near Cauchy and trend southwards. A point about midway between the extremities of the new rill appears to be situated approximately in N. lat. 12°, W. long. 36°.

Kempston, Beds, 1888, Aug. 18. THOS. GWYN ELGER.

Meteor Notes.

The Perseids of the present year have been apparently less numerous than usual, though on Aug. 10 the sky was cloudy at many stations. Observations were made at Bristol on several nights, with the following results:—

Date, 1888.	Time of observation.	Duration.	Meteors seen.	Perseids.	Radiant of Perseids.	State of sky.
	h m h m	hrs.			° °	
Aug. 2...	10 50–13 20	2½	42	14	35 +54	Very clear.
5...	10 0–12 45	2¾	31	11	42 +57	A little haze.
8...	10 0–12 45	3	36	20	42 +57	Slight fog.
13...	10 0–13 30	3½	49	13	52 +57	Very clear.
14...	10 0–13 0	3	25	2	......	Rather foggy.

Mr. David Booth, at Leeds, watched the sky on July 27 between 10^h and $14\frac{1}{2}^h$, and recorded 16 meteors, of which 5 were Perseids, from a centre at about 20° +55°. On Aug. 2 the sky was much involved in fog; and looking towards the zenith, between $10\frac{1}{2}^h$ and $12\frac{1}{2}^h$, he only noticed 5 meteors—and none of them were Perseids. On Aug. 8 about 45 meteors were seen during an observation extending over 4½ hours; and 25 of them were Perseids, from a radiant at 42½° +57°. On Aug. 13 the position had moved to 51½° +56° (15 meteors). These figures are nearly identical with those derived at Bristol on the same nights. The displacement of the radiant of the Perseids is thus clearly confirmed.

Mr. G. T. Davis, of Theale, near Reading, recorded a number of meteor-paths on Aug. 2, 3, 5, 8, and 10, including many Perseids; and, on comparing his list with my own, I find there are 7 accordances, on Aug. 5 and 8. Six of these were evidently Perseids, and the remaining one appears to have been an Aquarid from a radiant at 319° −12°. The heights of this meteor and of five of the Perseids average 68 miles at first appearance and 51 at disappearance. One Perseid, not included in this result, fell from a height of 43 to 28 miles, and was therefore much nearer the Earth's surface than usual.

The observations at Bristol between Aug. 2 and 14 revealed several of the numerous minor streams which are usually active at this period. I determined radiants, chiefly on Aug. 13, at 8° +33°, 23° +36°, 104° +79°, and 312° +15°, and there were traces of showers at many other points. Two brilliant meteors were seen here, viz.:—1. On Aug. 8, at $12^h\ 3^m$, travelling from 302° +56° to 320° +55¼° with a very slow motion, and leaving a bright train of sparks; 2. On Aug. 13, at $11^h\ 33^m$, falling from 113° +57½° to 126° +52½° with great speed, and leaving a streak for 40 seconds. The former meteor was as bright as Venus, and probably belonged to a radiant at $291^\circ + 52^\circ$. The latter was more brilliant than Venus ever is, and blazed out with sufficient lustre at the end of its course to illuminate the whole sky. It was a Perseid of the largest kind, and several interesting descriptions of its path and appearance have been received. It was not only seen at Bristol but at Leeds also by Mr. Booth, at Dublin by Mr. Monck, and at Birmingham by several observers. Mr. Booth's attention was attracted to the meteor by the vivid flash it gave. He was facing the N.E. sky at the time, looking for Perseids; but the sudden illumination caused him to turn, when he at once detected a brilliant streak projected on the stars of Draco from $275^\circ + 67^\circ$ to $260^\circ + 53^\circ$, and it remained visible for 3 minutes in a distorted form. The fireball appears to have passed over Yorkshire at heights differing little from the mean elevations of such bodies. W. F. Denning.

Bristol, 1888, Aug. 18.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Discovery of Encke's Comet.

Gentlemen,—

In your note to Mr. Lynn's communication on Encke's Comet in the 'Observatory,' No. 137, you remark that the Comet will hardly be observed before the middle of July. With the help of Drs. Backlund and Seraphimoff's ephemeris this interesting object was detected here on the evening of the 8th instant. The comparisons were hastily made while the Comet was close to the horizon, which gave the following as the approximate position:—

July 8, $6^h\ 34^m$, R.A.$= 8^h\ 47^m\ 56^s$, Decl.$= 12^\circ\ 49'\!\cdot\!5$ N.

This is almost exactly the position assigned to it in the ephemeris. The Comet was shut out from observation with the 8-inch telescope by a portion of the Observatory buildings, but in the 4½-inch instrument it appeared as a small bright nebulous star without a nucleus. As it is moving pretty rapidly both east and south, it will soon be well situated for observation.

Windsor, N. S. Wales,
1888, July 11.

Yours faithfully,
John Tebbutt.

The Discovery of Titan.

Gentlemen,—

It is well known that Titan, the sixth satellite of Saturn in order of distance, was discovered by Huygens on March 25, 1655. But it appears by some letters which have been recently published in the first volume of that elaborate work, the 'Œuvres Complètes de Christiaan Huygens,' that a claim was put in by Dr. Wallis for the discovery, not on his own behalf, but on that of Sir Christopher Wren and Sir Paul Neile. This claim appears to have been withdrawn or, at any rate, dropped; but its circumstances are of some interest, and though they seem to reflect discredit on Wallis, they cannot be ignored, now that they have been published.

All readers of astronomical books are aware that Huygens, in imitation of Galileo, at first claimed whilst concealing his discovery by expressing it in an anagram, sent to various persons. Amongst others, it was sent to Wallis in a letter dated June 13, 1655. It ran thus:—admovere oculis distantia sidera nostris, vvvvvvvcccrrhnbqx. The tract, "De Saturni Luna Observatio Nova," announcing the discovery, was published at the Hague on March 6, 1656; and on the 13th of the same month, in another letter to Wallis, Huygens explained the anagram to mean "Saturno luna sua circumducitur diebus sexdecim horis quatuor." But long before that, Wallis, on June 21 (July 1, N.S.), 1655, had sent Huygens a much longer anagram, consisting entirely of detached letters, thus:—a a a a a $\overset{9}{a}$ a a a
$\overset{1}{b}$ c c $\overset{5}{c}$ c c d $\overset{4}{d}$ d d e e e e $\overset{9}{e}$ e e e e $\overset{1}{f}$ $\overset{1}{h}$ i i i i i i $\overset{12}{i}$ i i i i i l $\overset{3}{l}$ l m m $\overset{6}{m}$ m m m
n n n $\overset{6}{n}$ n o o o $\overset{7}{o}$ o o o p p $\overset{5}{p}$ p p $\overset{1}{q}$ r r r r r $\overset{11}{r}$ r r r r r s s s s s s $\overset{13}{s}$ s s s s s s
t t $\overset{8}{t}$ t t t t t u u u u u u u u $\overset{17}{u}$ u u u u u u u u $\overset{1}{x}$.

When Huygens explained his own anagram, he expressed himself very anxious to know the signification of that propounded by Wallis. The latter accordingly explained it in a letter dated Oxford, March 22 (O.S.), 1656, stating that it meant "Saturni Comes quasi lunando vehitur. Diebus sexdecim circuitu rotatur. Novas nuper Saturni formas Telescopo vidimus primitus. Plura speramus." In a note which Huygens affixed to this letter, however, he says:—"Prolixum hoc anagramma commentus erat Wallisius, ut postquam meum exposuissem, eundem sensum ex suo illo elicere posset. Sed fraudem istam viri alii ingenui, ex Societate Regia, inter quos Moraius, mihi fassi sunt, jam ante suspectam." In a subsequent letter, dated April 17, 1656, Wallis tells Huygens that, in showing his tract announcing the discovery of a satellite of Saturn to Sir Paul Neile, through a mutual friend, Christopher Wren, they informed him that they had already seen it several times (long, Wallis said, he believed) anterior to the discovery by Huygens on March 25, 1655. No particulars, however, are mentioned.

Was this claim, then, really all a fraud? If so, it is certainly highly discreditable to those or him (if Wallis only be to blame) concerned in it. Let us hope with Miss A. M. Clerke, in a recent number of 'Nature' (for June 28), that some unexplained circumstance underlies the transaction. It is certainly very remarkable that Wallis, a fortnight after he received Huygens's anagram, should have constructed another (longer one) containing practically the same information. If he had really founded it upon Huygens's discovery, how had he arrived at the knowledge of that discovery? All must hope that further light will yet be thrown upon this matter. But it is well known that Wallis was skilled in the art of deciphering, in which he had been employed during the Civil War.

Yours faithfully,

Blackheath, 1888, July 18. W. T. Lynn.

The Solar Eclipse of October 24, 1688.

Gentlemen,—

I have, I think, more than once pointed out in the 'Observatory' that, when we find a statement in an ancient author that the Sun was obscured, we must be cautious in assuming that the obscuration was the result of a solar eclipse. But I have lately met with an instance that similar caution is necessary even in modern history before the days of the 'Nautical Almanac' and the general diffusion of astronomical information.

In the well-known diary of Evelyn, under date October 14 (24 new style), 1688, which was the king's (James II.'s) birthday, whilst the arrival of the Prince of Orange was shortly expected, it is remarked that the Sun was eclipsed at its rising. Now the eclipse on that day (which, it appears by Oppolzer's Canon, was annular in South Africa and the southern part of the Indian Ocean) was not visible in any part of Europe, so that Evelyn, misled perhaps by having heard that an eclipse of the Sun would occur somewhere that day, must have imagined that a darkness in the early morning, really caused by the state of the atmosphere, was due to that eclipse. The mistake has been, like many others, copied into several books without examination.

Yours faithfully,

Blackheath, 1888, Aug. 3. W. T. Lynn.

Earliest recorded Lunar Eclipse.

Gentlemen,—

Clouds and stormy weather prevailed yesterday morning, and precluded all observation of the commencement of the Moon's eclipse. On occasions of this sort one is induced to look back and inquire into the first-recorded instance of such phenomena. While the first lunar eclipse of which there is any mention in our own land would seem to be that of A.D. 690, recorded in the 'Annales Cambriæ,' we may go back for the first hint of a lunar eclipse to an extremely distant date. In searching for this, the following passage from Dr. Birch's 'Egypt' (Ancient History from the Monuments) is worthy of notice. Speaking of the times of the Old Empire, B.C. 3000 to B.C. 2000, he states that Nebka, the Necherophes of the Greek lists, "is said to have reigned 28 years; but the statement that the Libyans revolted in his reign, shows that at a prior period they had been subjected to the arms of Egypt. A sudden increase of the Moon's size, apparently a lunar eclipse, which occurred at moonrise, so terrified the Libyans that they submitted again to the rule of Egypt." The other passage hinting at some very early record of a lunar eclipse is the following, in Lindsay's 'Chrono-Astrolabe':—"Gaubil states that he found an account of an eclipse of the Moon in a fragment of an ancient book that had escaped the burning in the 35th year of Wan Wang on the day Ping-tszi (13th of the cycle), and this must have been in B.C. 1136, on Jan. 29, near midnight."

Faithfully yours,
S. J. JOHNSON.

Melplash Vicarage, Dorset,
1888, July 24.

P.S.—θ Libræ, which was to be occulted by the Moon 21 minutes at Greenwich on the 18th, afforded an interesting case of the effect of parallax. At $8^h\ 44^m$ the star was close to the terminator. It then ran along the bright edge of the Moon in such proximity to the latter and with such diminished light from the brightness that it was impossible to say whether it disappeared or not for 25 seconds.

On the Brightness and Density of Binary Stars.

GENTLEMEN,—

In 'The Observatory,' No. 120, I laid down a formula for comparing the brightness and density of binary stars in cases where the orbit had been determined and the total light photometrically ascertained, on the assumption that the smaller star was insignificant in mass compared with the larger. In the article referred to I used the term mass instead of density. This was not quite accurate, though I think my meaning was rendered sufficiently clear by the context. The figures arrived at by my formula, and given in the accompanying table, represent the relative amounts of light emitted from each unit of surface, on the assumption that each binary system was practically reduced to a single star of spherical shape and constant density.

I now desire to point out the consequences of supposing that the mass of the smaller star is not insignificant compared with that of the larger. I find that the only effect would be to increase the total amount of light-giving surface by a quantity dependent on the relative masses of the two stars which form the system, and which will therefore be different in different cases. Denoting this quantity (or rather its reciprocal) by x, the formula for the relation between the brightness and density of two binary systems becomes

$$\frac{k_1}{k_2}=x\cdot\left(\frac{I_1}{I_2}\right)\cdot\left(\frac{P_1}{P_2}\right)^{\frac{4}{3}}\cdot\left(\frac{a_2}{a_1}\right)^2,$$

where k, I, P, and a have the same meanings as in my former article. It only remains to determine within what limits x may vary.

The extreme change takes place when the two stars which form the system are of equal mass. In this case, supposing them to be spheres of the assumed density, the amount of surface presented by each will be to that presented by a single sphere equal to the pair as $(\frac{1}{2})^{\frac{2}{3}}$ to 1, and the amount of surface presented by both will be double this, or as $\sqrt[3]{2}$ to 1. The illuminated surface will thus in the extreme case be increased in the ratio of 1·26 to 1 nearly, and of course the intensity of the light from each unit will be diminished in the same ratio, or will be to the intensity computed on the former hypothesis as 0·79 to 1. As this may occur with either binary system, the value of x in the foregoing formula may range from 0·79 to 1·26, but cannot fall below the former figure or above the latter.

By adopting as a standard not the actual pair ξ Ursæ Majoris, but an imaginary pair fulfilling all the same conditions, but in which the smaller star is known to be insignificant, the range of the multiplier x as applied to the table which I gave in the article referred to is only from 0·79 to 1·00 (but it must be recollected that the brilliancy of the real pair ξ Ursæ Majoris may now be as low as 0·79 instead of forming the unit of the scale). Such a multiplier would not materially alter the results arrived at in the table. The brilliancy of γ Leonis might descend from 93·29 to 73·96, but it would still be nearly double the maximum brilliancy of Castor, which stands second in the list (though under the influence of the unknown multiplier it might have to descend to third place, giving the second to δ Cygni). The extremes in the table will still differ in brilliancy in the ratio of several hundreds to one.

I do not, of course, mean that the light emitted from each surface-unit is several hundred times greater in the case of one binary system than of another. The difference may not be a difference of light-giving power, but a difference of density; and recollecting the extremely low density which Professor Pickering and Mr. Maxwell Hall have on different grounds assigned to Algol (probably one star of a binary system), it may be conjectured that differences of density enter into the solution of the problem. My figures (as modified by the new factor x) give the relative light-giving power of each surface-unit on the hypothesis that the density is the same. If we suppose the light-giving power to be constant while the density varies, the stars will stand in the same order (those composed of the lightest materials being highest on the list), but the figures will no longer express the relative density, since that depends on the cubical content, not the amount of exposed surface. Probably density and illumination vary. Some sphere-shaped stars are heavier than others relatively to the radius of the sphere, and some stars of equal density emit greater amounts of light from the unit of surface. Nevertheless the results at which I have arrived may not prove wholly devoid of astronomical interest.

Yours faithfully,

Dublin, 1888, August. W. H. S. Monck.

Absolute and Relative Parallax.

GENTLEMEN,—

Writers in using the word *parallax* often do not make the necessary distinction between the *absolute* and *relative* parallax of the stars. For instance, Mr. Hall, in 'Monthly Notices,' vol. xlvii. p. 542, says of β Centauri that Dr. Gill "found a small negative parallax; hence the parallax must be very small." Now this was merely a *relative* parallax, and it therefore means that the comparison star had relatively a *small positive* parallax; as the amount of this was, however, within the errors of observation, all that the observations show is that β Centauri and its comparison star are practically at the same distance from the Sun, and, so far as appears, that distance may just as well be a small one as a large one, and the stars may be a pair accompanying each other through space.

This consideration is based on the supposition that there is no special evidence against these stars being physically connected together. When there is much difference in proper motion between two stars it is reasonable to assume that there is no connection between them; although even then is it not possible that in some instances they really are connected, and that even a large difference in proper motion may be due to the revolution of one star round the other? For β Centauri the proper motion is small, and though I do not see that Dr. Gill states that the proper motion of the comparison star has been determined, probably it is also small; therefore we may judge from the researches of Proctor and others that the likelihood of this star being really near β Centauri is very great, and its observation need give no information as to the absolute parallax of β Centauri. Yours faithfully,

Sunderland, 1888, August 21. T. W. BACKHOUSE.

Naked-eye Comets.

GENTLEMEN,—

Perhaps since Mr. Johnson remarked on page 266 that Sawerthal's comet, if visible to the naked eye, would make the seventh within the last seven years, he has so seen it. I find it is the *eighth* comet I have seen with the naked eye during the period named. It may be interesting to give some particulars of these.

In the following Table are given the dates when I first and last saw the respective comets with the naked eye, as well as the maximum lengths of their tails (with dates) as so seen. No doubt some of them would be visible for longer periods than the duration given, especially from the south of England, where, owing to the shorter twilight and other causes, they may often be longer seen than here.

Of course it may be that other comets besides these might have been seen by the naked eye in England with closer observation. Town-light in the direction of Comet 1885 *e* (Barnard) was probably the cause of my not seeing it with the naked eye, for it was certainly bright enough.

No. for reference.	Comet.	First seen with naked eye.	Last seen with naked eye.	Duration.	Greatest length of tail.	Date.
1	1881 *b* (Great)	1881, June 29	1881, Sept. 26	89 days.	20½°	1881, July 2
2	1881 *c* (Schaeberle's) ..	1881, July 27	1881, Aug. 28	32 "	10¼	1881, Aug. 19
3	1882 *a* (Wells')	1882, May 11	1882, June 4	24 "	1	1882, May 29 June 1
4	1882 (Great)	1882, Oct. 1	1883, Feb. 11	133 "	33	1882, Oct. 30
5	1883 *b* (Pons-Brooks').	1883, Nov. 19	1884, Jan. 28	70 "	6¼	1884, Jan. 21
6	1885 *d* (Fabry's)	1886, Mar. 29	1886, Apr. 27	29 "	38	1886, Apr. 26
7	1886 *f* (Barnard's)	1886, Nov. 9	1886, Dec. 28	49 "	27½	1886, Nov. 29
8	1888 *a* (Sawerthal's) ..	1888, Apr. 7	1888, May 14	37 "	5⅓	1888, Apr. 11

The place of observation is Sunderland, except that No. 1 was first seen at Tyndrum (Perthshire), last at Scarborough, and its greatest length of tail at Newhaven (Sussex); and Nos. 7 and 8 were last seen near Darlington and at Falmouth respectively.

Comet Sawerthal is now a very indefinite object—very much elongated, its head (if it can be said to have one) passing gradually into its tail. Its whole length last night was at least 30′, as seen with a 4½-inch refractor.

Yours faithfully,

Sunderland, 1888, Aug. 5. T. W. Backhouse.

The Canals on Mars.

The return of this planet to opposition during the past spring has again called attention to the strange changes which take place upon its surface, and more especially to the network of dark lines,—the so-called "canals"—with which, according to Prof. Schiaparelli, its "continents" or brighter areas are overspread. The great distance of the planet and its consequent small apparent diameter have prevented many observers from attempting the work of delineation, but those who have examined it seem to have been rewarded by observations as striking as those which have been chronicled in former years. Dr. Terby has observed a large number of the canals, as he has already stated in these pages ('Observatory,' No. 138, p. 298), and although our English observers do not appear to be able to recognize that peculiar hard-and-sharp configuration which Prof. Schiaparelli has ascribed to them, yet several have detected markings corresponding more or less faithfully to some of the canals in general direction, if not in character. M. Perrotin at Nice has not only observed them, but in a few cases their "gemination" or doubling also. But Prof. Schiaparelli has been more fortunate still, and on May 13 he detected, in the region to the north and west of the "Fontana Land" of Mr. Green's chart, a curious network of lines which he describes as "une espèce de triangulation." On May 20 six canals were observed in the region of "Mädler Continent," and were seen so well that Prof. Schiaparelli could make out "des très petites ondulations dans leurs deux bords, *qu'on pouvait distinguer l'un de l'autre.*" On June 2 and 4 the definition was better still, and he writes "Les mémorables journées de 1879–1880 et de 1882 étaient revenues pour la première fois, et enfin je revoyais ces images prodigieuses" (the canals, that is, and their duplications) "qui se présentaient dans le champ comme une exquise gravure sur acier, relevée de toute la magie des couleurs. Le 650 ne suffisait plus à l'épuisement complet des détails." And besides these canals in the equatorial regions, a minute canal has been detected by M. Perrotin in the north polar ice-cap, and apparently two or three by Prof. Schiaparelli. It is possible, however, that we have here observations corresponding rather to those made by Mr. Green in 1877, when he saw the south polar cap fringed by some bright points which were apparently fragments of the polar cap left behind in its contraction, than to the discovery of actual channels across the northern snows.

Several ingenious theories have been suggested to account for the canals, it being generally accepted that Mars possesses seas, islands, and continents, polar ice-caps and snows, atmosphere, clouds, and storms, closely resembling our own. Mr. Proctor regards the canals as rivers, but in so doing seems to ignore their most characteristic features, their straightness, their frequent intersections, and the fact that they invariably connect the seas together, running from one to another. M. Fizeau explains them by the analogy of the rifts in glaciers, and considers the entire planet as involved in a glacial epoch. But M. Perrotin has succeeded in tracing some of the canals from the ocean of the southern hemisphere, near the equator, right across both continents and seas up to the north polar ice-cap. If this observation be confirmed,—for as yet it stands alone, though M. Perrotin's skill in observing and superb instrumental equipment render his testimony of the highest order,—it will evidently refute both the theories just noticed. But a further difficulty in accepting the quasi-terrestrial view of the condition of Mars, strong as many of the arguments in its favour are, is found in the marvellous "inundations" which it compels us to believe are in continual progress on that planet. Thus this very season M. Perrotin has observed the flooding,—if the observation is to be so interpreted,—of a district larger than France, and its partial recovery shortly after. The district in question is Schiaparelli's "Libya," "Hind Peninsula" in Mr. Green's chart. Prof. Schiaparelli and Dr. Terby do not, however, admit that Libya entirely disappeared, though they record that it had become of a darker tint than it has usually shown, and that the expansion of two neighbouring canals had considerably reduced its area. Still, changes as great have often been noticed in other regions of the planet in previous years, so that there is nothing incredible *per se* in M. Perrotin's observation. But the drawings he has published, showing the various appearances of the district in question, have a further interest and importance, since very few drawings, if any, have hitherto been published actually showing, as do these, the general system of Schiaparelli's canals and the gemination of some of them. And they at once suggest the remark that the canals are seen, just as on Schiaparelli's Mercator chart, as hard *straight* lines, and this in whatever part of the disk they may be situated. It need hardly be pointed out that actual straight lines on the surface of the planet could only appear to be straight when seen near the centre of the disk. Everywhere else they would be seen as curved; near the limb they would appear strongly curved. It is therefore impossible to accept these canals, *as delineated*, as representing real markings on the surface of the planet.

The drawings by Prof. Schiaparelli in 'Ciel et Terre' for Aug. 1 are not open to this objection to the same extent, but one or two canals are shown as straight which should have been curved.

This inference opens up again the question of the reality—the true objective existence—of the canals. Dr. Terby, in 'Ciel et Terre' for Aug. 1 and 16, argues strongly for them, and adduces many examples of the observation of individual canals by different skilful observers—Holden, Knott, Schmidt, Secchi, and others. In some cases, however, this evidence is rather against than for the Milan astronomer; for whilst several observers have seen markings corresponding in position to some of Schiaparelli's canals, most of them have described them as of an altogether different character. The reluctance some astronomers have shown to accept the system of canals has not been based simply upon the fact that they themselves had not seen them, but that they had seen other markings in the same positions which they could not reconcile with them. Thus, for example, the long narrow marking known as "Nasmyth Inlet" has been frequently sketched by Green, Knobel, Lockyer, and many others; and it seemed to them as one of the most distinct and definite markings on the planet, but little inferior in darkness to the "Kaiser Sea," into which it flowed. But it has disappeared entirely from Prof. Schiaparelli's 1882 chart, and its place is taken by a straight narrow canal, utterly unlike it in appearance, and no broader or more distinct than a dozen other canals in the neighbourhood—less broad and distinct than some.

Something of this discordance may be put down to the stiffness and want of gradation of Schiaparelli's mode of drawing, to which so many observers have objected, and of which Mr. Proctor says ('Monthly Notices,' Vol. xlviii. p. 308):—"No one who has ever seen Mars through a good telescope can accept the hard and unnatural configurations depicted by Schiaparelli"*. But it appears to be Schiaparelli's testimony that he has himself witnessed the actual change from the more familiar appearance of the planet to these strange reticulations. In a letter dated June 7, addressed to Dr. Terby, he says:—"Mais ce qui est plus extraordinaire et inattendu, ce sont les changements survenus depuis un mois dans la Boréosyrte et dans les régions environnantes...... Quel étrange enchevêtrement! Que peut signifier tout cela? Evidemment la planète a des détails géographiques fixes, semblables à ceux de la terre, avec golfes, canaux, etc., à plan irrégulier. Vient un certain moment, tout cela disparait pour faire place à ces grotesques polygonations et géminations qui, évidemment, s'attachent à représenter approximativement l'état antérieur, mais c'est un masque grossier, et je dirai presque ridicule" ('Ciel et Terre,' Aug. 1, 1888, p. 285).

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* Prof. Schiaparelli's description of the canals seems hardly reconcilable in one important point with his delineation of them on his charts. He speaks of them ('Observatory,' Vol. v. p. 222) as never less than 2° of a great circle in breadth. This would correspond to about $0''\cdot4$ in a close opposition like 1877, or to $0''\cdot2$ in a distant one like that just passed. But they are drawn as only 1° on the charts. The canals, the two borders of which he could distinguish from each other, were but $0''\cdot1$ in breadth if drawn to scale on his 1882 map.

It seems impossible to accept this as a description of a real objective change taking place upon the actual surface of the planet, though as a record of a subjective appearance it must be unhesitatingly received; Prof. Schiaparelli's advantages in the way of keen and trained eyesight and telescopic and atmospheric definition are beyond challenge. Hitherto the puzzle has received no satisfactory solution, for Mr. Proctor's suggestion that the canals are rivers is quite irreconcilable with the account Prof. Schiaparelli has given of the appearances he has observed (see 'Observatory,' Vol. v. pp. 222 and 223). But it is quite likely that his further suggestion that they are "optical *products*," neither objective realities nor optical illusions, but phenomena of diffraction, may prove more satisfactory. Further observations are urgently required to test the point—observations not confined to two or three favourable nights near opposition, but begun early and ended late, and carried on with the most persistent continuity. And it is much to be desired that, so far as possible, the actual sketches made at the telescope should themselves be published, and not charts prepared from them, however full of detail. It will not be long, if this be done, before the present mystery of the Red Planet receives its solution.

Blackheath, 1888, Aug. 28. E. W. MAUNDER.

OBSERVATORIES.

EDINBURGH.—Prof. Piazzi Smyth has resigned the directorship of this Observatory and the position of Astronomer Royal for Scotland. Over and above the natural regret one feels at the retirement of a capable man from active service, it is distressing to hear from himself the special reason which has induced him to take this step—to wit, "despair of ever being able to do anything good, or compete with other observatories, when the Government continue to refuse to do what their own Commission recommended." He has published the facts on which he bases this grave charge in a recent pamphlet, addressed to "all who love the Royal Observatory, and earnestly desire the hearty promotion of first-class practical astronomy in Scotland," and giving a faithful impression of the long heartbreaking struggle with the inscrutable ways of Governments and Government clerks, of which, unfortunately, others besides the venerable Professor have had dire experience. "The buildings and instruments of the Observatory," he says, "were reported on in the summer of 1876 by the Home Office Commission under Lord Crawford, and declared to be so much behind the times that the Observatory should forthwith be knocked down, rebuilt in a manner more suitable for modern astronomy, on a better site, fitted with new instruments, and the observers encouraged with more respectable salaries." Since then Her Majesty's Treasury have somewhat improved the house of the resident First Assistant, and acknowledged an accumulated claim for current repairs and expenses which had been paid by Prof. Smyth from his private purse, with the stipulation, however, that no further expense is to be incurred in that direction. Beyond this nothing has been done to carry out the suggestions made in 1876 except to appoint committees of enquiry, which have confirmed the report of the first; and the history of these Committees has been somewhat remarkable. In Prof. Piazzi Smyth's own words :—

"The Board of Visitors continued to apply to Government as represented by the Home Office" (for an increase of the available income of the Observatory to a point sufficient to finish, maintain, and work the equatoreal) "until in 1876 the then Home Secretary, Mr., since Sir Richard, now Lord Cross, adopted the following expedient for escaping the terms of agreement under which the Astronomical Institute of Edinburgh had parted with their observatory to Government nearly thirty years previously. That is, declining to listen to the long time accredited Board of Visitors, he appointed autocratically a Committee of his own to come down from London and examine and report on the case. That Committee accordingly arrived in July of 1876, examined at the Observatory, sat and discussed in Queen St., and then reported for a series of financial improvements of a similar, though altered character to those of the Board of Visitors, mainly because including a rebuilding of the Observatory in a modern manner and upon a new site. But the Home Secretary thereupon declined to listen to his own Committee, and neglected all their recommendations, as well as those of the older Board of Visitors. The venerable Mr. Duncan M'Laren, then senior M.P. for Edinburgh, moved thereupon in Parliament to have the Committee's Report publicly printed, which was done in 1877. Still, however, nothing came of it until 1879, when on account of further representations by the same watchful guardian of Scotch interests, the Home Secretary found it expedient to send another of his Committees to examine and report again. Confining itself however this time to the Equatoreal, and without admitting the Astronomer to their Council, this Committee advised certain improvements, obtained a grant for executing them, and handed it over to the Office of Works, where it is believed either to remain still, or to have lapsed to the Treasury after doing little or nothing at the instrument."

The assistance required by the observatory is of two kinds—first, a sufficient sum should be spent on the fine equatoreal to make it workable, for at present it is clumsy to work, too big for its dome, too complicated, and fitted with a poor spectroscope; and, secondly, reasonable salaries should be paid to the astronomers. The late director has obviously spent a great deal of thought on the most suitable and economical method of rendering the equatoreal efficient, and gives the results of his labours in the present pamphlet. To enable the disinterested to judge of the fitness of his claims on the second count, he, among other things, quotes two advertisements which appeared simultaneously in the daily papers of 1880 December 6:—

> An open competition is to be held simultaneously in London, Edinburgh, and Dublin for the post of Second Assistant Astronomer at the Royal Observatory of Edinburgh. Salary £100 per ann. (Tempered, however, in fact with an extraneous and temporary addition rising by £10 per ann. to £50 per ann., so long as certain extra work is carried on.)

> An open competition will shortly be held for two Junior Clerkships in the Colonial Office, with salaries commencing at £250 per ann. and rising to £600. Five of the Junior Clerks have additional emoluments. The higher clerkships, with salaries from £700 to £1000, are filled by promotion from the Junior Class.

Such comparisons have been made more than once; but there is no reason why the odiousness should diminish with age.

We believe the reconstruction of the organisation of the Observatory is under consideration by Government, being dealt with in the "Scotch Universities Bill"—a remarkable document which the united erudition of the Scotch Universities is said to be unable to translate into English or common sense. What will eventually happen to the vacant chair and the Observatory generally is therefore somewhat problematical; but the Government seem to have repented of their acquisition of 30 years ago, and intend to transfer the care of the Observatory to the University. Apparently it cannot fare worse than in the hands of Government; and the recent example of Oxford, as showing how generously a University can act as guardian to an observatory, encourages us to hope devoutly that the transfer may be effected.

NATAL.—In his report for the year 1887, Mr. Nevill (better known by his late name of Neison) explains that no funds were provided during that year for the payment of salaries, and that the work of the Observatory would consequently have been entirely suspended, excepting for his personal exertions, had it not been for the gallant assistance of four ladies, who have attended to the time signal service and the meteorological instruments. We hope that such a condition of affairs will not long continue, and that work will soon be recommenced with redoubled vigour.

YALE UNIVERSITY.—Dr. Elkin has an extremely interesting report for the year 1887–8. He has now completed his heliometer observations on the parallaxes of the ten first-magnitude stars, and finds these results:—

Star.	Parallax.	Prob. Error.	No. of Comp. Stars.	No. Obs.	Proper Motion.
	″	″			″
α Tauri..........	+0·116	±0·029	6	64	0·202
α Aurigæ........	+0·107	047	2	16	0·442
α Orionis........	−0·009	049	2	16	0·022
α Canis Minoris ..	+0·266	047	2	16	1·257
β Geminorum	+0·068	047	2	16	0·628
α Leonis	+0·093	048	4	15	0·255
α Boötis	+0·018	022	10	89	2·287
α Lyræ..........	+0·034	045	2	30	0·344
α Aquilæ	+0·199	047	4	16	0·647
α Cygni	−0·042	047	4	16	0·010

the probable errors including an estimation of the probable *systematic* error, and appearing, therefore, larger than those usually assigned to such results. As regards accordance with existing results, the value found for Procyon agrees well with those of Auwers (+0″·240 ±0″·029) and Wagner (0″·299 ±0″·038); that for Altair with W. Struve's value +0″·181 ±0″·094; and that for Aldebaran with Hall's value +0″·102 ±0″·030; but O. Struve found very different parallaxes for Aldebaran (0″·516 ±0″·057) and Capella (+0″·305 ±0″·043); and the seven independent determinations of parallax of α Lyræ which have previously been made agree fairly well in assigning to it a parallax of about +0″·17. Dr. Elkin hopes to trace the source of these discordances. The well-determined minute parallax of Arcturus is remarkable as compared with its large proper motion.

The mean parallax of these ten stars, allowing for that of the 8th mag. comparison stars, is thus

+0″·089 ±0″·015,

which accords well with the values found by Gyldén (0″·084) and Peters (0″·102), though, in view of the large range of values, the term "mean" must be used with caution.

Dr. Elkin is now engaged on a triangulation of the region near the pole, to get fundamental places of 24 stars; and in conjunction with Dr. Gill he will this winter observe the opposition of Iris for determination of solar parallax. We congratulate him upon the completion of a good piece of work.

WOLSINGHAM.—In Circular No. 21, dated Aug. 25, Mr. Espin says:—"On Aug. 13 a remarkably bright line, apparently F, was observed in the spectrum of R Cygni. The observation was confirmed on Aug. 22, and by Dr. Copeland the same night, who measured the line. Dunér observed this star (1879 Mar. 16, 1880 May 14, 1882 Oct. 6), and found a weak III-type spectrum. An extraordinary change would seem to have taken place in this star."

PUBLICATIONS.

Transactions of the Seismological Society of Japan.—Volume XII., recently published, contains several very interesting papers. From one of them we quote the following passage:—"The first systematic application of the theory of vibrations to the problems of earthquake motion was made by Hopkins in his Report on the Theories of Elevation and Earthquakes, presented to the British Association in 1847. During the forty years which have elapsed since then, our knowledge of earthquake phenomena has steadily grown. The labours of Mallet have been largely supplemented by the observations and experiments of a small army of enthusiasts who have pitched their tents on the trembling soil of Italy and Japan. Their energies have been mainly directed to the perfecting of seismographs and seismometers, to the registering of all kinds of earth movements, to the study of the effects of these on buildings, and, in a limited degree, to the measurement of the velocities of propagation of disturbances due to artificial earthquakes. With all this activity on the experimental side we have to confess that theoretic views have hardly advanced beyond the stage at which Hopkins left them in 1847. G. H. Darwin's discussion of the strains due to continental areas, and Lord Rayleigh's investigation into a special case of surface-waves on an elastic solid, are perhaps the only mathematical pieces of work that have any distinct bearing on seismic phenomena." Of the enthusiasm of the inhabitants of Japanese tents the volume before us is a ready witness, though, as above hinted, the work mainly deals with the experimental part of the subject. That the want of attention to the theory is not the result of lack of interesting problems to solve is demonstrated by Mr. John Milne in an able paper on "Certain Seismic Problems demanding Solution." The sound phenomena are curious and not finally explained, though three of the papers deal with this point, from one of which, written by Dr. Knott, we have quoted the first paragraph; there are many anomalies in the velocity of propagation, which is (*e.g.*) greater the greater the initial impulse, and diminishes with the distance from the centre; the period of vibration is subject to unexplained changes; the character of the motion observed is by no means in accordance with existing theories, and so on. Seismology is a science which we have, perhaps fortunately, few opportunities in England for studying practically, but perhaps some of our younger mathematicians could find time to supplement the admirable experimental work of shakier stations by undertaking the requisite investigations.

NOTES.

The Hongkong Standard Clock.—Dr. Doberck has kindly sent us the account of an analysis he has made of the rates of the sidereal standard clock at his observatory. "It was put up in 1884 and stopped in August 1885. It was then started, and allowed to go from 1885 September 7 till 1887 October 7 without being touched except for winding. Its rate during short periods was very regular, and practically perfect during a series of astronomical observations made during one evening; but in order to examine its rate during longer intervals the rates were determined for intervals of 10 days, the first being from 1885 September 7 to 17. It was found impossible to represent the rates without terms depending upon the time, as the rate showed considerable alteration, which, however, slowly decreased. Even then the observed values of the rate exhibited fluctuatious which might be represented by aid of harmonic analysis. The barometric coefficient appears to amount to only a quarter of its usual value, but has been omitted owing to the uncertainty arising from the regularity of changes of temperature with changes of pressure. The humidity of the air changing likewise with temperature, no attempt has as yet been made to compute a small corresponding coefficient. The daily rates are well represented by the formula

$$\text{rate} = -0^{\mathrm{s}}\cdot56 - 0^{\mathrm{s}}\cdot00655\,t + 0^{\mathrm{s}}\cdot00000420\,t^2 - 0^{\mathrm{s}}\cdot063\,(\theta - 70^\circ),$$

where t is the time in days from 1885 September 12, and θ is the temperature in degrees Fahrenheit."

From the table which Dr. Doberck supplies we learn that the observed mean daily rate agrees fairly well with this theoretical rate for the whole period of over two years. On comparing the mean daily rate and the theoretical rate for each of the 76 ten-daily periods into which the whole period of observation is divisible, the difference is found to be under $0^{\mathrm{s}}\cdot10$ for 36 of these periods, between $0^{\mathrm{s}}\cdot10$ and $0^{\mathrm{s}}\cdot20$ for 27, between $0^{\mathrm{s}}\cdot20$ and $0^{\mathrm{s}}\cdot30$ for 10, and over $0^{\mathrm{s}}\cdot30$ for the last 3 periods only, when the terms depending on the time doubtless begin to require modification. The errors of the clock, counted from its theoretical indication, were positive for the first 23 periods, reaching nearly 5 seconds as maximum; negative till the 32nd period, reaching 10 seconds as minimum; positive till the 56th period, reaching 8 seconds; and then negative, reaching 15 seconds as minimum, till near the end of the period discussed.

THE ABERRATION OF THE FIXED STARS.—The question has been raised as to whether aberration is independent of the motion of the source of light; that is, whether the constants of aberration found from two stars near together, but with different proper motions, are the same. W. Struve found a systematic difference between the values of this constant deduced from observations of Polaris and those of α^1 Ursæ Minoris. Prof. Nyrén has discussed observations of the difference of right ascension between these stars made at Pulkowa since 1841 by three observers, and finds a very satisfactory accordance between the deduced values of the constant of aberration for the two stars. The discrepancy noticed by W. Struve must then be due to some other cause; and he has traced it to an inaccurate formula used in the reduction.

THE ORBIT OF MERCURY.—In No. 173 of the 'Astronomical Journal' Mr. O. T. Sherman discusses the meridian observations of Mercury made at Washington (1861–1882), Paris (1856–81), Radcliffe (1864–77), and Greenwich (1864–1883). He finds that the residuals of observation as compared with the tables point to a periodic variation in orbit as follows:—

"It will be seen that the planet receives, when between 330° and 360° of true anomaly, a disturbance whose immediate effect is a considerable disturbance of its orbital velocity, so that the planet passes through perihelion later than was expected. The secondary effect appears to be to draw the planet nearer the Sun and increase its orbital velocity; so that the planet passes beyond its calculated place near aphelion, passes its aphelion slightly before the calculated position, and suffers a secondary effect of a similar character in passing from aphelion towards perihelion. Although this action is such as to suggest a strong opinion as to its cause, I beg to delay the discussion thereof till I shall present certain relations which are found to exist among the elements of Encke's comet."

PROF. C. A. YOUNG has contributed to 'Scribner's Magazine' for July an extremely interesting account of his summer trip of last year, when he visited Europe with the ultimate object of observing the Solar Eclipse in Russia. Those who were not fortunate enough to meet him may gather some idea of his wonderful energy by glancing through this account of his doings in a few months, during which he visited the observatories near London and Paris, at Strassburg, Munich, Vienna, Prague, Berlin, and Pulkowa, made extensive preparations for observing the eclipse, which ended disastrously through no human fault, and wound up with the Meeting of the British Association at Manchester and a visit to Birr Castle, each journey from one place to another involving sight-seeing by the way. And the sight-seeing, as he gives an account of it, has been so thoroughly intelligent that he has recalled in very few words the prominent features of each place in turn. There are some very good illustrations: may we be forgiven for mentioning that the woodcut of Greenwich is accidentally reversed?

ENCKE'S COMET, *b* 1888, was first observed by Mr. Tebbutt (see his letter on p. 338) on July 8, but the first announcement to reach Europe was from the Cape Observatory, where it was found independently on Aug. 3, on which date the correction of Drs. Backlund and Seraphimoff's ephemeris was $+8^{s}$ in R.A. and $-1'{\cdot}3$ in Decl. No further ephemeris has reached us.

COMET *c* 1888 (BROOKS).—On Aug. 7 Mr. Brooks discovered a Comet of which the following ephemeris for Berlin midnight has been published by Dr. Kreutz in Astr. Nach. No. 2854:—

1888.	R.A. h m s	Decl. ° ′	Log *r*.	Log Δ.
Sept. 2 ..	13 19 43	+36 0	0·0386	0·1906
6 ..	13 43 35	33 8	·0543	·1967
10 ..	14 5 19	30 8	·0703	·2051
14 ..	14 25 4	+27 4	·0864	·2157

FAYE'S COMET, *d* 1888, was found at the Nice Observatory on Aug. 9. For the following ephemeris for Greenwich midnight we are indebted to Mr. Hind:—

1888.	R.A. h m	Decl. ° ′	Log. Δ.
Sept. 1	5 0·8	18 22	0·2582
5	6 10·8	17 54	·2515
9	6 20·5	17 23	·2449
13	6 29·9	16 50	·2383
17	6 39·0	16 14	·2317
21	6 47·8	15 35	·2251
25	6 56·4	14 55	·2184
29	7 4·5	14 13	·2117

CONNAISSANCE DES TEMPS 1890.—A copy of the 'Connaissance des Temps' for 1890 was presented to the Paris Academy on Aug. 13, *i. e.* nearly 3 months earlier than the issue of the preceding volume. The principal changes are as follows:—a new polar star about 10^h R.A. has been added; the semidiameter and duration of transit of the Sun and the parallax and aberration are given for every day; and tables for the calculation of the phases of solar eclipses for all parts of the Earth and for the visibility of Saturn's ring are added.

THE WEATHER.—In his 'Monthly Meteorological Magazine' for August Mr. Symons gives some interesting facts about the recent bad weather. Speaking of London alone, he says "from June 4 to July 31 in 1887 rain fell on 10 days; during the corresponding period this year it has fallen on 46 days. In 1887 the total depth during that period was 1·12 in.; this year it has been 7·22 in., so that rain has been nearly 5 times as frequent and 7 times as heavy." For 30 years back there is only one July wetter than the last, viz. July 1880, when the fall was 5·11 in. This year it was 4·91 in. There have, however, been several Junes and Augusts wetter than this in recent years. "We must look further into details if we wish to know why it has been so uncomfortable. Much depends upon how the rain falls, whether in continuous drizzle or in torrential rains. Now though we have had several short storm rains, we have not once had an inch in 24 hours, but it has fallen day after day, so that in July it rained on 26 out of the 31 days."

Another and very important consideration is the temperature, and this has been far more remarkable for lowness than the rain for excess of quantity.

Mr. Symons's figures are as follows:—

Mean temperature at 9 A.M.:—Mean for 30 years 64°·5; 1888, 59°·2.

Absolute maximum during the month:—Mean for 30 years 86°·0; in 1888, 75°·9.

Average maximum for the month:—Mean for 30 years 74°·7; in 1888, 67°·3, and there is no other so low, the lowest being 1879, 67°·7.

Average minimum for the month:—Mean for 30 years 54°·0; 1888, 52°·3.

Absolute minimum for the month:—Mean for 30 years 45°·2; 1888, 42°·8.

PARALLAX OF POLARIS.—Prof. Pritchard has just completed his photographic determination of the parallax of Polaris, and obtained the following results. Stars *a* and *b* are nearly N. and S. of Polaris, and the distance *ab* is 2307″; and *x* and *y* nearly E. and W., and distant 2816″.

From star	(*a*),	mag. 11,	parallax	0″·099 ±0″·014
"	(*b*),	" 7,	"	0 ·043 ±0 ·015
"	(*x*),	" 8½,	"	0 ·076 ±0 ·015
"	(*y*),	" 8½,	"	0 ·062 ±0 ·016

It is remarkable that the differences are in the same direction as the probable relative parallax of the comparison stars, though perhaps greater in amount.

WE are glad to hear that, according to information received, H.M. Treasury have sanctioned the provision of two telescopes which shall enable the Royal Observatories at Greenwich and the Cape of Good Hope to take part in the International scheme for charting the heavens by photography. This decision has been somewhat tardy; but the intervening time has perhaps not been wasted by being employed in experimental work. Among other matters, Professor Stokes, the Astronomer Royal, and Sir Howard Grubb have very carefully considered the best form for the photographic object-glasses which will be required for the work, a question involving several quite new points in practical optics, but which they hope will soon be decided.

M. BOUQUET DE LA GRYE stated, at the meeting of the Paris Academy of Sciences on Aug. 20, that the Bureau des Longitudes have reported to the Government in favour of the unification of time in France, Algiers, and Tunis. Steps will probably soon be taken to carry out this project.

OUR readers will see from another part of this Number that Prof. Piazzi Smyth has resigned his position as Astronomer Royal for Scotland, which he has held for 43 years.

PLANET No. 278 has been named Adelheid.

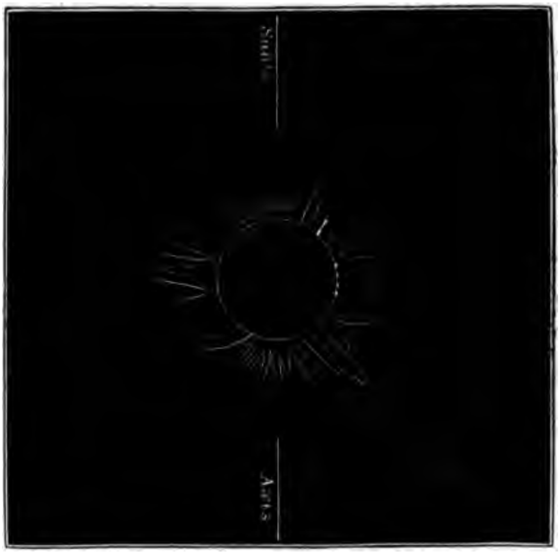
THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 141. OCTOBER. 1888.

The Corona of 1886.

In Professor W. H. Pickering's interesting report of his observations of this eclipse which has just been published in the 'Annals of Harvard College Observatory' is a shaded drawing, made from his three most successful photographs, which shows some specially interesting features.



The northern and southern rifts are widely open and fairly symmetrical about the Sun's axis. The rifts are bounded on each side by the usual groups of synclinal structure, which are well-marked but unsymmetrical, those on the eastern side of the corona being depressed towards the equator, while the corresponding groups on the west are nearly radial. On the eastern side the coronal extension is generally less than on the western, and the mass of equatorial rays is also much less broad than that on the west. The separation between the southern synclinal groups and the equatorial masses is very well-marked. The south-west synclinal group is narrow and conical, and extends to a greater height than any other part of the corona. Both polar rifts are filled with rays of the same character as the polar rays in 1878, but less fine and regular.

But by far the most remarkable features of the drawing are certain very faint extensions on the western side of the corona. From the summit of the narrow, conical, south-west synclinal mass mentioned above, a narrow jet rises to a height of 48′ from the limb; it then divides into three parts, the most conspicuous one falling over towards the left, another portion turning to the right, while the third reaches an elevation of 60′ and then falls back towards the left. A similar jet extends from the ray immediately to the east of the cone, curving towards the ray previously described. The equatorial mass to the north of the cone is prolonged into a broad structure, which curves over to the left, as if returning to the solar surface in the region surrounding the great prominence. Portions of this structure extend as far as 60′ from the limb. These remarkable extensions are indicated by dotted lines upon the diagram. They are somewhat difficult to make out in Prof. Pickering's drawing; the description is therefore taken from his report.

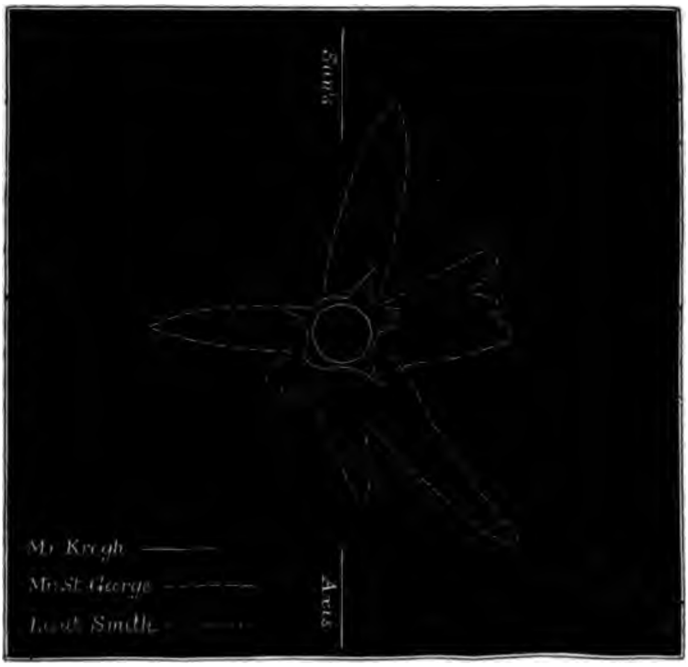
It is obvious that if they are genuine coronal features they are of the utmost interest, appearing to show that matter can rise from the Sun to a height of nearly two solar diameters, and fall back again upon its surface. The highest ray completely curving over that I have seen upon any other coronal photograph is only about 13′ in height. Indeed these extensions are higher than any ray previously photographed, except perhaps in Janssen's negative of 1883.

On comparing Prof. Pickering's drawing with the photographs taken by the English observers it appears to fairly represent the general appearance of the corona. The English negatives show far more detail in every part; the drawing, however, is intended as a diagram, and does not profess to show more than the main features. But the closest scrutiny of the English photographs fails to show the slightest trace of the great extensions.

The extensions are not indicated in the eye drawings given by Prof. Pickering, and are shown upon one only of the American plates, which, it seems from the Report, was taken with a camera having a 4-inch portrait-lens, the exposure being 8 seconds. Owing to an accident to the apparatus a second image of short exposure was superposed on the first, causing the Moon to appear slightly elliptical. In the photo-mechanical print (apparently the size of the original) from this negative, given in the Report, the shorter diameter of the Moon is only $\frac{3}{16}$ths of an inch. The corona is an irregular blur, extending to about 75′ from the limb, but the details of the extensions cannot be made out in the print. On the whole, considering the small scale and the imperfections of the image, it seems hardly safe to assume the reality of these remarkable features on the evidence of a single photograph, though possibly an examination of the original negative might lead to a different conclusion.

The image of the Moon on Messrs. Maunder and Schuster's photographs is about $\frac{6}{10}$ths of an inch in diameter, and if the great coronal extensions were shown upon them, they would reach nearly to the extreme edge of the plates, where the film is injured in some cases. It would obviously be advisable in future to use plates of sufficient size to include distant extensions, and care should be taken to avoid imperfections of the film, even beyond the usual coronal limits.

Professor Pickering in concluding his report appears to recommend instruments of small aperture for coronal photographs. I believe him to be quite correct as far as extension is concerned, indeed the smaller aperture has sometimes given the greatest extent of corona, as in the present case and in 1878. But the coronal details cannot fail to be better shown with an equally good image of a larger size.



Mr. Turner has kindly furnished me with particulars of the eye drawings made by two of the English observers. The accompanying diagram (drawn to half the scale of the previous one) gives the outlines of these; the outer dotted line represents Lieut. Smith's drawing, the second Mr. St. George's, and the innermost contour is from a sketch by Mr. Krogh, of the American party, given in Prof. Pickering's Report. Lieut. Smith made use of a disk of $93'$ in diameter, that covered the brighter portion of the corona (which is all that is shown in most of the photographs), and also rendered his eyes more sensitive by blindfolding them for 20 minutes before totality. It will be seen that, while no indications are given of Prof. Pickering's curling jets, the extent of the corona is far greater than that given in the photographs, Mr. St. George's drawing extending to four times the Moon's diameter and Lieut. Smith's to seven times. The general form of the corona appears pretty much the same in the different sketches, but the extension is different, according to the sensitiveness of the eye.

Mr. Krogh's drawing agrees well with the form of the corona as photographed; the others are not quite so accordant, but considering the great difficulty attending such drawings, they present a very fair general agreement. Mr. Turner points out that the great extensions shown in the sketches could not well be due to mistakes of scale, as special attention was paid to this point, and scales of concentric circles at distances apart equal to the Moon's diameter were prepared for the drawings.

W. H. Wesley.

Inō Tadayoshi, the Japanese Surveyor and Cartographer *.

Inō (originally Jimbō) Kageyu † was born in 1744 in a small village called Sawaramura in the province of Shimōsa, Japan. Inō was the name he acquired by marrying into a family, in accordance with the very usual Japanese custom.

His father-in-law was a *sake* brewer, conducting a business which had descended from father to son for many generations. On his death affairs were found to be in a very bad state. Inō thereupon applied himself diligently to the business, and through his untiring efforts, combined with strict economy, he gradually amassed considerable wealth. In his fiftieth year, that is about 1794, he transferred the whole business to his son and began his scientific career.

Astronomy was the study to which he devoted himself. The books at his disposal were all in Chinese and contained many obscure passages which he in vain tried to understand. Ultimately he made his way to Yedo, and sat at the feet of the Takahashis, father and son, astronomers to the Shōgun. The elder Takahashi died in 1804, and it was with the younger Takahashi that Inō had most to do. Certain letters written to him by Inō still exist, and their style is such as would naturally be used by one addressing a former teacher.

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* From the 'Journal of the College of Science, Imperial University, Japan,' Vol. ii. pt. 3.

† Inō is the family name, and Kageyu the common name. His *real name*, used only on important occasions, was Tadayoshi, or Chūkei, as some pronounce it.

In 1800 Inō set out, by permission of the Government, to survey the Island of Ezo at his own expense. In the following year he was instructed to survey all the coasts and islands of Japan. The survey of the north-eastern coast was finished in 1804, and by 1818 his labours in the field were completed. It should be mentioned, perhaps, that certain parts of the coast were surveyed very imperfectly—such as the eastern and the north-western coasts. Exactly when he died is not known certainly, but for some time after the completion of the survey he seems to have been engaged in the construction of his maps.

The instruments which Inō employed in the survey were destroyed by fire; but in 1828 two instruments, said to be exact copies of the original ones, were made by Ōno Yasaburo, the father of the late engineer who constructed the Mint at Ōsaka. These are now in the possession of the Meteorological Office. A compass-needle, made and used by Inō, has, however, been preserved by his family.

Ōno's instruments are two, one for measuring azimuths and the other for measuring altitudes. The former is simply a horizontal circular disk of copper 19 inches in diameter, graduated by radial lines into degrees. Seven concentric circles are traced near the extremity of the disk at such distances apart that, when a straight line is engraved joining the point where the inmost circle cuts a given radial line to the point where the outmost circle cuts the next radial line, this so-called diagonal gives by its intersections with the intermediate circles angular intervals corresponding to 10′ or one sixth of a degree. The graduated circular disk rests on three legs provided with levelling-screws. From its centre rises an upright wooden pillar which is surmounted by a tube (or perhaps a telescope) for sighting distant objects. The levelling of the circle is accomplished by means of a brass "plummet" hanging down one side of the upright pillar. The pillar rotates freely, and carries with it a horizontal rod resting on the graduated circle. The position of this rod indicates at once the angle to be read.

The instrument for measuring altitudes is a brass quadrant, 19 inches in radius, with a telescope fixed to one of the straight limbs. The whole is mounted on an upright wooden pillar resting on three legs. The telescope and quadrant, which move together in a vertical plane about a pivot passing approximately through the centre of gravity, can be clamped in any required position. From the angle of the quadrant a "plummet-line," in the form of a brass rod, hangs. The position of this rod, as it hangs just free of the quadrant arc, indicates the angle to be read. The quadrant is graduated in a manner very similar to the azimuth circle, only to a finer degree of division. The radial lines measure to thirds of a degree; and by means of the "diagonal-scale" arrangement, angles might be read to half-minutes. On the azimuth circle again it would be difficult, if not impossible, to read to minutes even.

With such instruments did Inō carry out his survey. About 1135 direct measurements of latitudes were taken by means of the quadrant. The distances between successive stations were measured by three distinct methods. Ropes were used as our land-surveyors use chains; also a kind of wheel or roller, the number of revolutions of which measured the distance travelled. Then with the azimuth instrument in combination with the compass, a triangulation by means of prominent hills and land-marks was carried out. From the distances so obtained the longitudes seem to have been calculated.

The results of Inō's labours are given in the "Dai Nippon Enkai-jis-soku-roku," or the "Record of the True Survey of the Coasts of Japan" (1821, 14 volumes). This treatise existed simply in manuscript till 1870 (Meiji, 3), when it was published in proper book form by the Tōkyō University (Hitotsu-bashi)—at that time known as the Daigaku Nankō. Three kinds of maps were constructed, the largest consisting of 30 different sheets, the medium-sized of two, and the smallest of one. These maps have been the basis of all subsequent ones; and for many places in Japan Inō's measurements of latitude (and longitude) are the only ones which have as yet been made.

On completion of the survey, Takahashi published an epitome of the results in a book having the title "Inō's Table of Latitudes and Longitudes." From certain remarks in the preface to this work, it would appear that Inō rather doubted the truth of the magnetic variation, and was inclined to refer its appearance in Europe to carelessness either in the construction or handling of the compass-needle. There can be little doubt, however, as to the accuracy of Inō's own observation that in Japan at that time the mean direction of magnetic north coincided practically with the direction of geographical north.

According to Inō the mean length of one degree of latitude is 28·2 *ri*. From a copy of the standard *shaku* used by Inō—the original seems to have been lost by fire—this distance has been estimated as equivalent to 110·7 kilometres. The true value is 111 kilometres. The lengths of a degree of longitude in latitudes 35°, 40°, 44° are given as 23·1 *ri*, 21·6 *ri*, and 20·285 *ri*, respectively. Reduced to kilometres, these are 90·7, 84·8, and 79·66. The true values are 91·08, 85·18, 79·99, differing in no case from Inō's values by as much as one-half per cent.

When we consider the age at which Inō began his scientific career—an age at which most men are thinking of retiring from the busy field of life—and when, further, we call to mind the rude instruments with which he did his work, we cannot but feel that we have here a man worthy of a high place amongst the scientific leaders of the last generation.. In these days of candid criticism, his work has stood the severest tests and remains a grand monument of his perseverance, patience, and accuracy. His greatness is now fully appreciated, and some six or seven years ago received Imperial recognition. The rank of Shō-shi-i, or Senior 4th class, was at that time conferred on him. Excepting nobles, very few held that rank in the days when Inō flourished, although it is common enough now-a-days. Such posthumous honours are, besides, very rare. A stone monument in his honour is very soon to be put up in Tōkyō.

In preparing this short biography of Inō, I have been fortunate in the hearty assistance of Mr. Arai, Superintendent of the Meteorological Office, and of Professor Yamagawa and Mr. Nagaoka of the Imperial University, without whose aid indeed I could have done little or nothing. C. G. Knott.

Motions of the Solar System[*].

No other hypothesis has been suggested which offers such direct and complete answers to most of the questions which relate to the origin, structure, and unity of the universe, as Newton's law of gravity. It is but natural, therefore, that the majority of the problems which arise in regard to the motions of the solar system should have their origin in an effort to confirm that law.

The first attempt to apply Newton's law to all the motions of the solar system was made by Laplace. When, however, Lindenau and Bouvard undertook to compute their tables of the motions of the planets, a complete revision of Laplace's theory was found necessary. So enormous is the labour involved, that there exists, besides those mentioned, only one other complete set of theories and tables of the motions of the principal planets,—that of Leverrier. Leverrier's tables of the inner planets are now nearly thirty years old. His tables of the outer planets are much later, having employed his attention almost to the day of his death. His tables of Jupiter and Saturn were published in 1876, and those of Uranus and Neptune in the year following. Newcomb's tables of Neptune were published in 1865; those of Uranus in 1874. Hill's theory of Jupiter and Saturn, which has for years occupied his attention, has at last been completed, and he is now engaged in preparing tables therefrom. These are intended to form a part of a complete series of tables of the principal planets now being prepared under the direction of Professor Newcomb at Washington. Another such series is also being prepared by Professor Gyldén at Stockholm.

The values of the coefficients of the terms of short period in the motions of the principal planets are now pretty well known; and the same might be said of the secular variations, were it not for the difference between theory and observation which exists in regard to the motion of the perihelion of Mercury, which was discovered by Leverrier, and has been confirmed by Newcomb, in a discussion of the observations of the transits of Mercury, extending over a period of more than two centuries. The cause of this difference still remains unknown. The completion and comparison with observations of the new theory of the four inner planets, now being prepared under the direction of Professor Newcomb, will be awaited with interest, with the hope that it may throw new light on this interesting subject.

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[*] Abstract of an Address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science, at Cleveland, O., Aug. 15–22, 1888, by Ormond Stone, Vice-President of the Section.

The only recent original tables of the Moon's motions are those of Hansen. These, like Leverrier's tables of the inner planets, are now more than thirty years old. These tables have been compared with observations, and agree fairly well with those made during the century preceding their publication, but not with those made before or since that time. The theoretical value of the acceleration of the Moon's longitude is 6″; that found by Hansen from accounts of ancient total eclipses of the Sun, 12″. Newcomb, however, considers these accounts as unreliable, and, limiting himself to the Ptolemaic eclipses of the *Almagest* and the Arabian eclipses of the *Table Hakémite*, obtains the value 8″·3, or, from the Arabian eclipses alone, 7″,—a value but little greater than the theoretical value. Dr. Ginzel, from an extended examination of accounts of ancient and mediæval total eclipses of the Sun, concludes that Hansen's value requires a change of only a little over 1″. His solution, however, in reality depends upon the ancient eclipses alone. The only other theory of the Moon comparable with Hansen's is that of Delaunay. This theory, however, is limited to a determination of the inequalities in the motion of the Moon due to the action of the Sun, on the hypothesis that the orbit of the Earth is a pure ellipse, and differs from that of Hansen in that the inequalities determined are not expressed numerically, but only symbolically in terms of arbitrary constants.

While the coefficients of the inequalities upon which Hansen's tables are based seem to be pretty well known, I am not aware that the tables themselves have been sufficiently checked, except by comparison with observations. Apparently the great desideratum now is a set of tables computed from Delaunay's theory in a completed form, or computed in some other way entirely independently of Hansen's. Until Hansen's tables are thus checked, it is questionable whether it can be safely said that the motion of the Moon cannot be completely accounted for by the law of gravity.

The detection of the two satellites of Mars by Professor Hall may be considered the most interesting recent achievement in pure discovery. It was not till the discovery of these satellites that a means was offered for the accurate determination of the mass of that planet. No satellites of Venus and Mercury have as yet been detected, and the values at present assumed for the masses of those planets are very uncertain.

In 1788, just one hundred years ago, Laplace published his theory of Jupiter's satellites. This theory is still the basis of the tables now in use. Souillart's analytical theory of these satellites appeared in 1881. The numerical theory was completed only within the last year, and the tables therefrom remain still to be formed.

Bessel made a careful investigation of the orbit of Titan; but the general theory of the Saturnian system, which he commenced, he did not live to finish. Our knowledge of the motions of Saturn's satellites, with the exception of Titan, was very meagre until the erection of the great equatoreal at Washington. A difficulty in the determination of a correct theory of the motions of Saturn's satellites is the fact that there are a number of cases of approximate commensurability in the ratios of their mean motions. The most interesting case is that of Hyperion, whose mean motion is very nearly three fourths that of Titan. In this case there is the additional difficulty that their distance from one another is only about one seventh as great at conjunction as at opposition.

Our knowledge of the motions of the satellites of Uranus and Neptune depends almost entirely on the observations made at Washington. Quite accurate determinations of the masses of these two planets have been obtained. The large secular motion of the plane of Neptune's satellite, to which Marth has called attention, needs confirmation.

The number of the asteroids is so great that they have been the frequent subject of statistical investigation. The systematic grouping of the nodes and perihelia which exists was shown by Newcomb to be the effect of perturbation. Glauser finds that the grouping of the nodes on the ecliptic is the result of a nearly uniform distribution on the orbit of Jupiter. Professor Newton had previously found that the mean plane of the asteroid orbits lies nearer to the plane of Jupiter's orbit than to the orbit-plane of any individual asteroid. Eighty-five per cent. of the asteroids have mean motions greater than twice, and less than three times that of Jupiter; and the mean motions of none approximate closely either of these, the two simplest ratios possible. The next simplest ratios lie beyond the limits of the zone; that is, there are no asteroids having mean motions nearly equal to or less than one and a half times that of Jupiter, and none nearly equal to or greater than four times that of Jupiter. The labour of determining the general perturbations and computing tables of an asteroid is as great as in the case of a major planet. It is no wonder, therefore, that tables have been prepared for scarce a dozen of these small bodies, and that these are already out of date.

Of well-known comets of short period, Encke's, which has the shortest period of any, possesses the greatest interest to the student of celestial motions, since it was from a discussion of the orbit of this comet that Encke detected evidence of the existence of a resisting medium which produces an acceleration in the comet's mean motion. This acceleration has been confirmed by the investigations of Von Asten and Backlund. The investigations of Oppolzer and Haerdtl indicate that there is an acceleration also in the mean motion of Winnecke's comet.

We have thus glanced briefly at the present condition of our knowledge of the motions of the principal bodies of the solar system. Only four cases have been found in which we cannot fully explain these motions, so far as known, by Newton's law of gravity. The unexplained discordances are the motion of the perihelion of Mercury, and the accelerations of the mean motions of the Moon and the two periodic comets just named.

If we go beyond the solar system, we cannot tell whether Newton's law does or does not apply without modification to all parts of the universe. It is principally in the hope of answering this question that double-star observations are carried on; and, in the case of the many binary systems already detected, Newton's law is satisfied within the errors of observation. Nevertheless, this evidence is purely negative, and its value, it seems to me, not at all commensurate with the labour expended upon it, unless it be in the case of such objects as Sirius, whose observation may assist in the solution of the problem of irregular so-called proper motion. The angles subtended are in general so small that relatively large personal errors are unavoidable; so that even though their motions be controlled by a law or laws of gravity widely different from that of Newton, it is not likely that such differences can be proved with any degree of certainty. It is rather to the study of the proper motions of the fixed stars and of the nebulæ, and then only after a lapse of hundreds and perhaps thousands of years, that we must look for a solution of this question. O. Stone.

Richard A. Proctor.

Alone and unconscious, in the ward of a New York hospital, there passed away, on the evening of September 12th, a man whose place in the astronomical world it will, just now, be hard, if not impossible, to fill. Richard Anthony Proctor, whose name as an expositor of science has become a household word wherever the English language is spoken, is dead, and the fertile brain and untiring pen which have done so much to popularize the results of the discoveries and investigations of himself and other scientific men are still for ever. Born in 1837, he became a student at King's College, where he came out either at or close to the head of the honours list in Theology, a subject for which he was untiring in the subsequent expression of his contempt. Thence he proceeded to St. John's College, Cambridge, where he fostered that taste for mathematics which was innate in him. He took his degree in 1860, coming out 23rd Wrangler. A few years after this he gave his first astronomical work to the world, 'Saturn and its System,' which was published in 1865 and contained the first popular account of Clerk-Maxwell's theory of the discrete nature of the Saturnian Ring-system. But it was distinguished by something more than this; it was not only an exhaustive treatise on the subject of that particular member of the solar system to which it was devoted, but further contained a mass of varied astronomical information which it is even now impossible to study without profit. Although at first commercially unsuccessful, this volume brought its author prominently before those best able to appreciate its merits, and in 1866 he was elected a Fellow of the Royal Astronomical Society. In 1872 he became one of its Secretaries and editor of the 'Monthly Notices'; but his, even then, too multifarious engagements (involving, *inter alia*, a visit to America) compelled him to resign those offices.

Among his subsequent works his 'Sun,' 'Moon,' 'Other Worlds than Ours,' 'Orbs around us,' and 'Universe of Stars' must be familiar to most of those who will read these lines, as containing expositions at once vividly clear and scientifically exact of the very latest results of study and investigation in the various subjects to which they are respectively devoted.

In speaking, though, of Mr. Proctor as a popularizer of science, it must by no means be supposed that he was a mere expositor of other men's results. He was an original investigator of no mean order, and it is not too much to say that it is to him that we are indebted for the final demolition of that theory of the Stellar Universe which so long held place in the text-books as Sir William Herschel's, and for the establishment of the doctrine of the relative ages and various stages of development of the different members of the solar system. Nor will his exhaustive study of the conditions of the last transits of Venus be forgotten, enabling him, as it did, to give to the world the best popular exposition of the nature and use of that phenomenon that has yet appeared.

Among other special subjects Charting was always a favourite one with him, and his expedients for improving and facilitating the construction of maps, both celestial and terrestrial, were as numerous as they were ingenious. Certainly for the purpose either of tracing the path of a planet or comet, identifying any star down to the 6th magnitude, or of learning the constellations, his 'Larger Star Atlas' and its reduction the 'New Star Atlas' are unsurpassed. His industry was prodigious. He may almost be said to have laid down his pen to enter the lecture room, and to have quitted the platform to resume his pen again. His acquirements were of a most varied nature; not only was he a high-class (and, as his 'Geometry of Cycloids' shows, an original) mathematician, but he was an earnest student of English literature, who knew Shakespeare and Dickens literally by heart. He was also a keen chess and whist player, and wrote constantly on the last-mentioned game. It has been said that the elephant's trunk can alike rend a tree and pick up a pin; a diversity of use surely akin to that of the hand which could at one time chart 324,000 fixed stars, and at another pen that amusing refutation of an American charlatan, "Who wrote Dickens's Novels?" in a recent number of the 'Cornhill Magazine.' But amid all his multifarious and heterogeneous work he kept one idea steadily before him, that of the production of an original treatise on Astronomy, which should form a standard embodiment of the astronomical knowledge of the last quarter of the nineteenth century. It is grievous to think that he was struck down in the midst of the work which he regarded as the crowning one of his life.

Nervously sensitive both to praise and blame, he has been accused by some people of being quarrelsome. To those who knew him personally no stupider libel was ever uttered. A more tender-hearted man never existed; he was feminine in his abhorrence of inflicting pain, and his fear of hurting any one's feelings was sometimes carried to an extreme. But for the impostor, or self-advertizing quack, he neither had nor tried to show any mercy; and there were few more scathing writers than he when he had to lay on the lash. Another stupid slander sometimes uttered was that he had no religion, than which nothing could conceivably be more false. For theology his contempt was supernal; but no one could be more reverent and religious than he who now lies still and cold so far from his native land. W. NOBLE.

A simple Electric Light for the Telescope.

AN oil lamp to illuminate the field of view in a telescope is nearly always a troublesome and frequently a dirty arrangement, and besides this its proneness to flicker with the slightest draught is often a source of great annoyance. Hence illumination by the electric light has for some time been a desideratum, but difficulty in arranging the necessary details has hitherto been a prominent hindrance to its extensive use. To meet this difficulty I venture to propose the following arrangement, which for many months has worked satisfactorily, is always ready for use, requires no skilled attention, and entails the use of no acids or fluids which with reasonable care need cause any injury to the delicate contents of an astronomical observatory.

As regards the battery, each cell, of which six are required, consists of a stoneware jar about eight inches in diameter and ten in depth. Seven rods of compressed manganese and carbon, one of which is capped, surrounded with a piece of canvas, form one element, a zinc cylinder just large enough to enclose the former constituting the other, the exciting fluid being a concentrated solution of sal ammoniac. It will thus be seen the battery possesses no particular novelty, for it is nothing more than a Léclanché cell arranged for quantity, by reducing the internal resistance. The large surface of metal, however, in this form is imperative, for if the ordinary rod of zinc be substituted it will be found that the carbons split when the circuit is closed for any long time. The remaining details will best be understood by referring to the accompanying diagram.

Fig. 1.

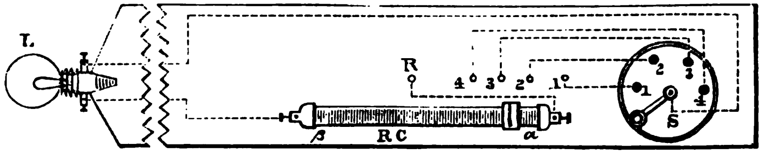
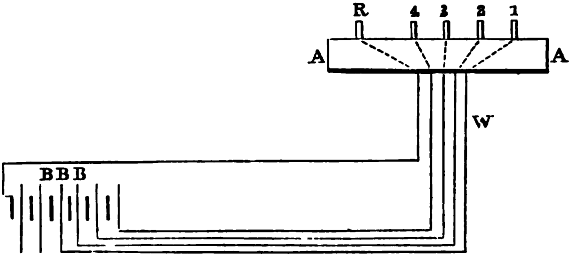


Fig. 2.



RC. Resistance-coil. S. Switch and handle. L. Lamp. A A. Piece of wood to attach batteries to telescope. B B B. Batteries. W. Cable of wires.

Fig. 1 shows the piece of wood which is permanently fixed to the telescope, the dotted lines giving the arrangement of the wires sunk in grooves well paraffined over. Fig. 2 illustrates the means of connecting the battery to the telescope, and consists of another piece of hard wood AA with a cable of five flexible wires, each of which terminates in a brass pin for fitting into a corresponding socket in the telescope board as shown in fig. 1. Wire 1 is connected with the third carbon, number 2 with the fourth, 3 with the fifth, and 4 with the sixth, the return passing from the first zinc to the pin marked R. No mistake can be made in placing these pins into their respective places, as it will be seen that the pins and their sockets are not regularly placed at equal distances, hence the wood AA can only be attached to the telescope in one manner.

Turning the switch on to the disc 1, puts on the light at its feeblest brilliancy, pushing the ring shown on the resistance coil RC from α to β increasing it to a small though sensible amount—a great convenience and one which equally applies in the arrangement no matter how many cells are in use. Moving the switch to the next plate 2 increases the light, and so on with the addition of each cell as the handle is shifted on to discs 3 and 4.

By employing this simple arrangement it will be seen that the only thing to be done when requiring the light is to fix the brass pins in connection with the battery into their respective places on the telescope board and to turn the switch. In actual practice it is only necessary to use three cells at first, adding the others gradually as required: but it is always advisable to turn off the light at every available opportunity, as by so doing its durability is greatly prolonged. Inasmuch as each cell gives nearly two volts, a four or five volt incandescent lamp with one side blackened on its exterior, but provided with a looking-glass reflector within, is the one most suited for the purpose; but care should be taken in ordering the same that it does not require too much current, as the batteries being coupled in series do not give a greater output, after a little use, than about one and a half to two ampères. If the jars be kept quite full with a saturated solution, the batteries will recover themselves in twenty-four hours, ready for the next night's use; but if very frequent and prolonged use is to be made of the light, it is best to have two sets of six cells, and to use them on alternate nights.

It is needless to state that this arrangement is not presumed to possess any thing approaching novelty, but is merely offered to those who may be desirous of illuminating their telescopes without the slightest trouble and at very little expense, and who may not be acquainted with the necessary details, for it need hardly be added that all the parts can be purchased ready made, and that the whole arrangement can be put together, as it was in my case, without the employment of any skilled labour.

Ivy House, Clapham Common. EDMUND J. SPITTA.
Sept. 1888.

[Various simple forms of electric light have doubtless been tried by different observers; at Greenwich the "Schanschieff Battery Syndicate's" system has been used. The battery is a single fluid primary, composed of an exceedingly simple combination of carbon-zinc elements; the exciting liquid is a solution of the basic sulphate of mercury. When in action the mercury is precipitated out of the solution in its metallic state, and by the patented method it is easily reinstated into proper solution for fresh use. The carbon plates do not require renewing, and the zincs only at considerable intervals, as, being continually amalgamated by the action of the battery itself, they are only slowly consumed. When not in use the elements are held suspended out of the solution by a very simple arrangement, so that the battery may be worked intermittently, or may remain out of use for any period of time without loss of electrical energy or consumption of materials. The internal resistance of the cell being small and its electromotive force high (1·4 volts), together with its constancy and freedom from fuming or giving off gas while in action, make it especially suitable for lighting and other purposes, either for scientific or domestic use. The cost of the solution being 5*s.* per gallon, and 4*s.* per gallon being allowed for the returned residue, it is essentially a cheap source of electrical energy. The battery, which with one charge will run 10 hours, is contained in a case 9 × 8 × 5 inches, and weighs 9½ lbs. The prime cost of the apparatus is about £2.—Eds.]

Meteor Notes.

Observations were made here on the nights of Aug. 29 and 30, Sep. 6, 7, 8, 9, and 10. 102 meteors were seen during watches extending over 13½ hours. No conspicuous showers were noticed, indeed the paths of the meteors registered were notable rather for their diversity than for their convergence upon definite points. In some previous years I have remarked the same circumstance that the meteors of September are very discursive in their flights, and that though a large number of showers are slightly asserted, there are very few streams displaying special activity. This is also applicable to some other periods of the year, and it makes it essential that the meteor observer should institute prolonged watches and be rigidly exact in recording data.

During my recent observations (Aug. 29 to Sep. 10) meteors occasionally fell from radiants at 313° +65°, 314° +26°, 354° +67°, and 33° +54°, and I also suspected showers at 45° +23°, 48° +63°, 59° +49°, 0° −3°, and 345° ±0°. The first of these I also saw on Aug. 25, 1887, at 311° +65°, and I have previously recorded several of the others.

Between July 26 and Sep. 13 the paths of about 150 meteors were registered by Mr. David Booth at Leeds, and he has referred them to the following radiants:—

July	340°	−12°	6 meteors.	Aquarids.
Aug.–Sep...	30	+18	11 „	Position diffuse.
August	339	+46½	4 „	Perhaps one shower of Lacertids.
August	332	+39	4 „	
Aug.–Sep...	293	+10	7 „	Aquilids.
Aug.–Sep...	3	+ 5	3 „	Meteors bright and slow, with thick trains.
September..	255	+55	5 „	Head of Draco.

The last of these was well observed at Bristol in 1885, Sep. 3–5, at 253° +54°.

Fireballs are more than usually abundant in September, though the month does not include any rich periodical display of meteors. The end of August appears to be similarly distinguished. This year a very fine meteor was seen on Aug. 25 at $10^h\ 23^m$, by Mr. P. H. Coventry, of Birkdale, Southport, "It was much brighter than any planet, and appeared equal to electric light at a few hundred yards. There was no train. Course from near ζ Herculis to α Coronæ; duration estimated at 2½ secs."

On the following evening (Aug. 26), at 8^h 18^m, a meteor, larger than Venus at her brightest, was observed by Mr. D. Booth at Leeds. It fell quickly in a vertical direction from about 216° +45° to 206° +39½°. Colour yellowish-white, nucleus pear-shaped. Mr. Booth remarks that it would have lit up the sky had it appeared a little later. This meteor was seen at Bristol, but clouds and strong twilight, combined with unpreparedness on the part of the observer, render the observation far less accurate than it would otherwise have been. At the latter place the meteor also fell vertically, the course being approximately from 108½° +55° to 107½° +46°. Motion very quick, and there was a momentary train. A comparison of the observations shows the radiant to have been in the zenith at about 278° +52½°. The meteor began at an average height of 66 miles over a point in Morecambe Bay, 6 miles N. of Fleetwood, and ended at an elevation of 41 miles after a perpendicular descent of 25 miles. The observed path assigned at Bristol gives the meteor a greater height than that at Leeds, as though observed earlier in its flight at the former place, which is quite possible. In such cases of discrepancy it is safest to quote the mean of the observations.

On Sept. 6, at 13^h 9^m, another brilliant meteor was recorded at Bristol. Path from 355½° +29° to 354¼° +40°. It burst out very suddenly, and must have exceeded Venus at maximum. The radiant was probably in Pisces.

On Sept. 7 two further meteors of exceptional size were seen by the writer. At 11^h 56^m a very fine flashing meteor passed from 128° +54° to 127½° +45°. It lit up the northern sky for a moment, and after three outbursts the nucleus lost its brilliant aspect, but it continued its track several degrees further (in the same line of motion), though in the form of a rush of red-hot ashes. There was a faint streak left for 5 seconds. At 12^h 23^m a very slow bolide crossed the meridian about 13° S. of the zenith. Path from 341° +40½ to 8° +34°. Near the end of its course the meteor evolved several fragments, and these, losing ground on the nucleus, soon formed a perfect string of bright red drops following in its wake. The nucleus itself slackened greatly at the end, and when on the verge of disappearance the beaded line, with its large, pear-shaped leader, had come nearly to a standstill. The whole phenomenon was so gradual, and so very distinct in its manifestation, that the eye was readily enabled to grasp the details, and this can very rarely be attained in regard to meteoric apparitions. As the meteor sailed along through Andromeda, it emitted a bright glow sufficient to obliterate the stars in this region; and at one time the nucleus blazed up considerably, so that I thought the appearance about to take rank with that of a fireball of the largest type, but it relapsed into that of a body varying between the lustre of Venus and Jupiter, being apparently weakened by the offcome of numerous fragments. Though not nearly so bright as some, it was certainly the most beautiful meteor I have ever seen. Its radiant-point was probably in the western sky, in the extreme S. region of Lyra or Hercules; but I have met with no other accounts of this interesting meteor, or of the two others seen on Sept. 6 and 7. The appearance of these fireballs at this particular epoch confirms the statement I made in the 'Observatory,' Vol. iii. pp. 128, 131, to the effect that Sept. 6–7 is a very favourable date for such phenomena.

On Sept. 13, at $10^h\ 45^m$, a fine meteor was noticed by Mr. Monck at Dublin. Its approximate course was from "about halfway between β Cygni and Altair (rather nearer the latter), and passed nearly by the Dolphin cluster, moving towards Pegasus, but disappeared soon after passing the cluster. Motion slow with a fine train of sparks, which, however, did not persist long enough to be of use in fixing its course."

Bristol, 1888, Sept. 15. W. F. DENNING.

Selenographical Notes.

ENCKE AND KEPLER.—A comparison between Schmidt's map of this interesting region and those of Beer and Mädler and Neison shows the desirability of a careful examination of the locality with a view to confirm the existence of certain prominent details drawn only by the former observer. With respect to the ring-plain, Encke, Mädler and Neison represent a small mountain on the north side of the floor connected with the south wall by a narrow longitudinal ridge and nothing more. Schmidt draws a double ridge—one nearly axial and the other adjoining it on the east; the first terminating on the north in a large crater abutting on the wall, and the latter including a smaller crater about midway between its extremities. He also shows two isolated mountains on the western quarter of the interior near the foot of the slope. If the formation be observed when E. long. of the morning terminator is from 42° to 45°, the double ridge of Schmidt may be easily distinguished, though the eastern member of it appears to be considerably nearer the wall than he shows it. The axial ridge, far more prominent, seems to bifurcate at a distance of two thirds its apparent length from the southern border, one branch running up to the north-west and the other to the north-east wall, thus inclosing a rhomboidal-shaped area, which answers to the large crater in this position in Schmidt's chart, though it does not display any resemblance to a true crater. As the Sun's altitude increases these features become rapidly obliterated by the brilliancy of the streaks emanating from the neighbouring Kepler; to such an extent that, when it amounts to 30° or thereabouts, nothing can be traced on the floor of Encke except a brilliant oval spot near the north-east wall and another, less bright, near the west wall. I have never seen the small crater on the eastern ridge shown by Schmidt, which probably, like other details in the interior, is very evanescent. At 9^h on June 19 a fine view was obtained of this neighbourhood with a power of 284, Encke showing as an oval ring filled with shadow, and only the south-western half of the wall of Kepler being in sunlight. At this stage the small crater on the outer slope of the eastern wall of Encke was conspicuous in the form of a round black spot without any visible rim. Under a higher angle of illumination, viz. from 9° to 12°, I have frequently seen this object (which Mädler and Neison appear to have overlooked) as a brilliant crater. Schmidt shows another craterlet on the south end of the axial ridge which has not, I believe, been as yet seen by other observers. A noteworthy feature in connection with Encke, though by no means confined to this formation, is the well-defined hexagonal shape assumed by the border a few hours after sunrise. When on the terminator, and for some hours subsequently, its contour is perfectly oval; but as the altitude of the Sun increases, the curvature of the western half of the ring gradually changes into three nearly straight sections, while on the opposite or eastern side a very similar transformation occurs, with the difference, however, that here the three sections are slightly curved. It would be useful to note approximately the stage at which this curious change takes place. On the plain about 30 miles due west of Encke are two contiguous craters, very prominent under an illuminating angle of about 10°. They are shown by Schmidt, but not by Mädler or Neison. Other small craters exist amid the many ridges between Kepler A and Encke B which are omitted in the smaller maps. About midway between the former crater and Kepler, Schmidt appears to have noted a short cleft, running in a south-easterly direction from a narrow ridge north of Kepler A to mountains lying south-west of Kepler. I can find no reference to this object in his text.

Schmidt's representation of Kepler is very true to nature. He alone shows the remarkable double wall (or, more properly speaking, terrace) which, soon after sunrise and till the Sun's altitude is 20° or more, may be traced nearly all round the ring-plain. Mädler and Neison represent the central mountain as of small dimensions, the latter describing it as "very insignificant." Schmidt, however, draws it as consisting of five or six distinct mounds occupying a considerable space on the floor. I can find no confirmation of the existence of the presence of so large a group of objects in the interior, though under favourable circumstances indications of more detail than is shown by Mädler and Neison are apparent.

The lofty ridges which encircle Encke on the west, south, and east, remind one, at sunrise, of the border of a vast walled plain, Encke itself resembling a large crater on its floor.

Kempston, Beds, 1888, Sept. 18. THOS. GWYN ELGER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Comet of 1666.

GENTLEMEN,—

In the second volume of Pingré's 'Cométographie' allusion is made to a statement by Robert Knox that a comet (or rather its tail) was seen in the island of Ceylon (Ceilan, as Pingré spells it) in the month of February 1666, but agreement is also expressed with the opinion of Struyck that there is an error in the year, and that the comet which appeared in the early part of 1668 was in fact the one referred to by Knox. I do not feel able to concur in this view, and will therefore with your permission call attention to the circumstances as related by him. His father (who bore the same name, Robert Knox) commanded the frigate 'Ann' in the Hon. East India Company's service, and in consequence of injuries sustained by the ship in a storm was compelled with a number of others to go ashore on the island of Ceylon in the year 1659, where they were detained as captives, but in other respects well treated. The father died there in the following year (February 9, 1660); the son, after being compelled to remain nearly twenty years on the island, succeeded at last in escaping and arrived in England again in 1680, and in the following year published a book giving an account of his adventures and a description of Ceylon and its inhabitants. In the course of his narrative he mentions the appearance of the comet of 1664, and the rebellion against the king of the island which occurred about that time and had, he seems to think, some connection with it. Afterwards comes an account of a later one, which I will quote in full:—"In the year 1666 in the month of February there appeared in this Countrey another *Comet* or stream in the West, the head end under the *Horizon*, much resembling that which was seen in England in the year 1680 in December. The sight of this did much daunt both King and People, having but a year or two before felt the sad event of a *Blazing-star* in the rebellion which I have now related. The King sent men upon the highest mountains in the Land to look if they could perceive the head of it, which they could not, being still under the *Horizon*. This continued visible about the space of one month, and by that time it was so diminished that it could not be seen."

Now the head of the comet of 1668 was observed at Goa in the East Indies and must have been visible in Ceylon during part of its appearance. Moreover the probability that there is a mistake of two years in Knox's narrative is much diminished not only by his giving the correct date for the comet of 1664, but also by the cometary appearances of the last few years. The comet of 1666 (if there were such) would seem by the above account of its appearance (resembling that of 1680) to have made a near approach to the Sun. So did that of 1668, two years afterwards. So did in recent years the comets seen early in 1880, in the autumn of 1882, and in the month of January 1887. The fine comet of 1843 also moved in an orbit very similar to those of the three just mentioned, and in all probability that of 1668 (but it was so imperfectly observed that this is only probability) did so too. It would appear therefore that there is a family of comets moving in nearly the same orbit, which carries them when in perihelion very near the Sun; and another member of it may have been visible in 1666. That on that supposition Robert Knox is the only person who gave an account of it will not seem so very remarkable if we consider how scanty are the records of the kind about that time of objects not visible in the northern hemisphere. Although this is chiefly conjecture, you may deem it not wholly devoid of interest.

Yours faithfully,

Blackheath, 1888, Sept. 10. W. T. Lynn.

Earliest recorded Lunar Eclipse.

Gentlemen,—

In his letter under the above heading in 'The Observatory,' No. 140, Mr. Johnson appears to endorse the view of Dr. Birch that the phenomenon which, according to Manetho, frightened the Lybians into again surrendering themselves to the rule of Egypt in the reign of Nebka, was, in fact, a lunar eclipse. Mr. Poole ('Encyclopædia Britannica,' 9th edition, vol. vii. p. 731) doubtfully acquiesced in this view, adding the appropriate qualification "unless it be legendary." It is certainly difficult to see how an eclipse could have suggested the idea of an *increase* in the Moon's apparent size. Reference to the passage in Manetho shows that he does not say that this occurred at moonrise. Brugsch Bey and Prof. Rawlinson are content to quote the expression in the original without remark. It is impossible to fix with any approach to certainty even the century in which Nebka reigned, or was supposed to have reigned, so that if we could accept the theory that a lunar eclipse was referred to, we should not have the means of determining its date.

Yours faithfully,

Blackheath, 1888, Sept. 24. W. T. Lynn.

Lunar Rainbow.

Gentlemen,—

A lunar rainbow is a sufficiently rare phenomenon to deserve recording. At $11^{h}\,23^{m}$ on the night of Aug. 28, a black cloud lying near the S.W. horizon, immediately beneath Altair, about 50° of the arc of a lunar rainbow came out distinctly. The corresponding portion of the arc was not visible. The Moon was about 1½ hour above the horizon in the E. The ray was tolerably broad, and somewhat resembled an auroral column, only more curved. At $11^h 29^m$ the rainbow vanished, owing to the cloud drifting away.

Perhaps one of the earliest records of this phenomenon in our own land is Hollinshed's mention of that on September 7, 1557, where he says, "at 7 o'clock at night, in a black rainy cloud in the west was seen a rainbow, the Moon in the east risen one hour before, and fair shining, and at the full the day before." In his description of that seen at Glapwell Hall (Derbyshire) on Christmas Day, 1710, the Rev. C. Clarke, in 'Hundred Wonders of the World,' speaks of a "remarkable and very unusual display of colours;" and says "what most surprised the observer was the largeness of the arc, which was not so much less than that of the Sun, as the different dimensions of their bodies, and their respective distances from the Earth, seemed to require; but the entireness and beauty of its colours furnished a charming spectacle."

Faithfully yours,
S. J. Johnson.

Vicarage, Melplash, Dorset,
1888, Aug. 30.

δ Cygni.

Gentlemen,—

In the 'Observatory,' No. 114, there are some interesting remarks by Dr. Terby and Mr. Sadler about δ Cygni. In consequence, on July 23 last, I determined to examine it about sunset with my equatoreal of 8 in. aperture by Cooke and Sons. With power 350, I at once glimpsed the minute companion, and afterwards saw it well with powers 445, 550. It is so long since I tried this star, that I had entirely forgotten the Position-angle, to which I did not refer till I had made a very careful estimation, resulting in Pos. = 320°. This agrees well with the measures by Dr. Copeland and Mr. Tarrant in the same number, and I consider shows conclusively that I saw the real companion.

Dr. Terby, in the paper mentioned above, gives a long list of stars from Σ and OΣ, delicate tests of his instrument; and as his aperture and mine are identical, I have examined many of them with great interest, and I have well seen all that I have looked at, the only exception being 25 Canum Venat.; but the star was very unsteady with an east wind, so I must try again.

Yours faithfully,
George Hunt.

West Dulwich, 1888, Sept. 10.

Sir William Herschel's Observations of Volcanoes in the Moon.

Gentlemen,—

In connection with Prof. Holden's interesting communication on this subject in the 'Observatory,' No. 140, I would point out that neither his observation of July 15 this year nor that which he quotes of Dr. Gerling, of Marburg, on July 4, 1843, describing the brilliant illumination of certain mountain-peaks near terminator, is quite "on all fours" with those of Sir W. Herschel on April 19, 20, 1787, and May 3, 1783, inasmuch as on the date of Herschel's first observation, the west long. of the morning terminator being about 61°, on the second 49°, and on the last 50°, and the so-called volcanoes being evidently, from his description, not far removed from the central meridian, they must have been situated at least 40° or more from the terminator, and consequently not due to the direct illumination of the Sun. Herschel's observations are usually accounted for by supposing that the objects he describes were normal features illuminated by earthshine; but I must confess that I never read the circumstantial account given of the phenomenon by so admirable an observer without feeling that the ordinarily received explanation is hardly satisfactory. He says—"These three spots are plainly to be distinguished from the rest of the marks upon the Moon; for the reflection of the Sun's rays from the Earth is in its present situation sufficiently bright, with a ten-feet reflector, to show the Moon's spots, even the darkest of them; nor did I perceive any similar phenomena last lunation, though I then viewed the same place with the same instrument." Are we quite justified in assuming that Sir William Herschel was "deceived?" It is true nothing quite similar has been recorded since his time, but it is also true that careful and systematic observations of the unillumined disk are rarely, if ever, made.

Yours faithfully,

Kempston, Beds, 1888, Sept. 14.

THOS. GWYN ELGER.

GENTLEMEN,—

In the 'Observatory,' No. 140, p. 334, is an interesting account by Prof. Holden of an observation of a brilliant illumination of a lunar mountain-peak on the 15th July last, perhaps rendered still more interesting from the instrument with which it was made. But the conditions as regards illumination under which this observation was made were so entirely different from those prevailing at the times when Herschel perceived his volcanoes in eruption that, however interesting the phenomenon may be in itself, I do not think it is permissible to draw any conclusions as to the analogy of the appearances in question. The feature noticed by Prof. Holden was simply the early illumination, though an unusually bright one, of a considerable mountain-peak by the *direct rays* of the morning sun, and whilst the surrounding country was still immersed in the darkness of night; and under these circumstances it is not uncommon for mountain-peaks to glitter with extreme brightness, doubtless partly on account of the great contrast then existing between the brightly lighted surface of the mountain and the surrounding blackness, a contrast which would certainly not be reduced by the employment of an instrument of such magnitude as the Lick refractor, but chiefly because the phenomenon is evidently dependent upon some peculiar angle of reflection and vision. Indeed, under similar conditions of illumination, I have seen the mountain Piton glittering with extreme brilliancy in a manner comparable to that described by Prof. Holden, and it would seem that it is this mountain which was observed by him and also by Dr. Gerling in 1843. But the "volcanoes" seen by Herschel were situate far within the dark part of the Moon at the times of the observations of this observer, so that they could have been comparatively feebly illuminated by earth-shine, as clearly appears from the account in the 'Philosophical Transactions' for 1787. It therefore does not seem possible to consider Herschel's observations as referring to apparitions of the kind narrated by Prof. Holden; nor, moreover, does it seem necessary to ascribe them to any abnormal or rare occurrence, since the explanation usually given of them seems amply sufficient; that is, to quote from 'Celestial Objects,' "It is now well known that the volcanoes, which Herschel I. and others thought they saw in activity, on the dark side, were only the brighter spots reflecting back to us the earth-shine of the lunar night with the same proportional vivacity as the sunshine of the day." One of Herschel's "volcanoes" has been identified with the brilliant Aristarchus, and this object would seem to have been noticed as presenting a similar appearance at a date long anterior to the time of Herschel, since it is stated in Breen's 'Planetary Worlds' that at the time of the first quarter "Aristarchus is seen as a bright point on the dark part of the Moon's disc, and Hevelius considered that it was an immense mass of sulphur and saltpetre, and was constantly burning."

On p. 336 of No. 140, Mr. Elger draws attention to the shallow ring north of Plato designated by Schroeter χ, and it may be of interest to state that I saw this object about 7 or 8 years ago almost exactly as represented by Schroeter, both in form and position. I have not at present access to my observations of that time, so am unable to give any further particulars.

Yours faithfully,

West Brighton, 1888, Sept. 21. A. STANLEY WILLIAMS.

The Radiant of the August Perseids.

GENTLEMEN,—

Mr. Denning has several times drawn attention to the long continuance of the August meteor-shower and to the radiant, varying in position, as observed on different nights; but I am not aware that these peculiarities have been corroborated by other observers. I wish, therefore, to allude to some notes* which I made during this year's return of the Perseids. Cloudy weather prevented anything like continuity in the work, and I had to be satisfied with observations on July 26, August 8, 13, 14, and 15; but these may be useful as far as they go. I have put the results into the following tabular form:—

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* These are in part quoted by Mr. Denning in the 'Observatory' for September, p. 337.

Date. 1888.	Hours of observation. h h	Duration. h	Perseids registered.	Perseid radiant. °	°
July 26	10 –14½	4½	5	20	+55
Aug. 8	10 –14½	4½	21	42	+57½
13	9½–14½	5	17	51½	+56
14	9¾–12¾	3	5	55	+59½
15	10 –12	2	6	58½	+59½

Comparing these observations for August 8, 13, 14, and 15 with those taken by Mr. Denning at Bristol in several previous years, the uniformity in the positions will be noticed:—

Bristol.			Leeds.		
1880, Aug. 8....	41°	+55°			
1887, Aug. 8....	43	+56	1888, Aug. 8..	42°	+57½°
1888, Aug. 8....	42	+57			
1888, Aug. 13....	49½	+57½			
1885, Aug. 13....	51	+58	1888, Aug. 13..	51½	+56
1888, Aug. 13....	52	+57			
1887, Aug. 14....	53	+57	{ 1888, Aug. 14.. 1888, Aug. 15..	55 58½	+59½ +59½
1877, Aug. 16....	60	+59			

It thus appears that the radiant constantly runs in an easterly direction.

I may add that my observations were made with every care. Though the directions of some of the meteors I registered were somewhat erratic with reference to the radiants adduced, the meteors near the radiant usually gave a sharply-defined centre; and I had little difficulty in fixing the central places given in the table. Taking into consideration the short irregular meteors, and classifying them with others of the same stream that did not come to a sharp centre, I am led to the conclusion that in all probability the Perseid radiant is several degrees in diameter each night during its activity.

Yours faithfully,

Leeds, 1888, Sept. 17. DAVID BOOTH.

OBSERVATORIES.

KEW.—Marine and terrestrial telescopes, binoculars, and opera-glasses can now be tested at the Kew Observatory for the following fees:—Navy telescopes and field-glasses, 2*s.* 6*d.* each; opera-glasses and pocket telescopes, 1*s.* 6*d.* each. The Superintendent reserves the right to decline to examine any instruments which he may consider not capable of attaining a sufficiently high standard.

BEN NEVIS.—From the report of the British Association Committee on the Ben Nevis Observatory, we learn that the rainfall for the year 1887 was 126 inches; usually the difference of temperature at the summit and base is about 16°, but in the dry season last year it fell to 7°. Considerable attention has been paid to observations of halos, St. Elmo's fire, and other natural phenomena. The Committee strongly advocate the erection of another observatory on the coast near Fort William, so that the observers at the two stations may coöperate in observing and predicting storms. A grant of £50 was voted towards the maintenance of the observatory.

DUN ECHT.—We hear that the Earl of Crawford and Balcarres has offered the instruments of his well-known Observatory near Aberdeen to the Government, on condition that the administration of the Royal Observatory of Scotland remains in the hands of Government.

The Observatory of Dun Echt is one of the most perfectly equipped private observatories in the world. There is an excellent transit instrument by Simms, similar to that at Greenwich, a fine 15-in. equatorial refractor by Grubb, a reflector by Browning, several very good spectroscopes, chronographs, and other instruments of value.

PUBLICATIONS.

HARVARD COLLEGE.—Parts III., IV., and V., of Vol. XVIII. of the 'Annals' have been published. Part III. contains Mr. Parkhurst's photometric measures of asteroids, which are of a most interesting nature, though so difficult as to give only qualitative results. A form of transit-photometer was employed, the time being noted from the transit of an object across a wire until it disappeared; but instead of using a wedge to gradually absorb the light a "deflector" of glass was employed for this purpose, the effect being practically the same as a gradual limitation of the aperture. The author sums up his results as follows:—

1. The phase correction cannot be neglected and is peculiar to each asteroid.
2. There may be, for certain asteroids, large errors from rotation.
3. In most cases, after the phase correction has been determined, the remaining unknown errors are less than the average variation of the fixed stars.

Part IV. deals with observations made during the Total Lunar Eclipse of 1888, Jan. 28. Besides 27 observations of occultations of stars, a careful search was made by photography for a lunar satellite; a large number of plates was taken, but on examining them it was considered that the Moon has no satellite brighter than the 10th magnitude, or about 200 metres in diameter. From other photographic determinations, the total light of the centrally eclipsed Moon was found to be to that of the full Moon as 1 to 1,400,000. "This is about twice the ratio that exists between the Full Moon and the Sun."

In Part V. Mr. W. H. Pickering discusses his observations of the Total Solar Eclipse of 1886 August 29 in Grenada. Owing to accident he arrived somewhat late upon the scene, with a large amount of apparatus which he could scarcely hope to completely organize, so that some of the results he hoped for were lost. But he obtained five photographs of the corona and one of the spectrum. From three of the former a drawing has been prepared, with which Mr. Wesley deals in another part of this Number. The photographs were also used to determine the brightness of the corona, by comparing the results produced by a standard light on a hitherto unexposed part of the plate. This was in fact one of the chief objects of the expedition; but serious difficulties came in the way. The atmosphere of Grenada was woefully damp and liable to set up decomposition in the photographic films; the party suffered an enforced delay of a month before they could return; and then the plates were seized and retained for some weeks by unsympathetic custom-house officials. In all it was some months before they could be developed, and then only two were found suitable for determination of brightness, and a discordance between the results obtained from the two seems to show that for one of them the exposure may have been one second instead of two. The following conclusions, however, are arrived at:—

	units.
Total visual brilliancy of the corona and surrounding sky	60
„ actinic „ „ „	700
„ visual „ Full Moon	4
„ actinic „ „	39·5
„ „ „ „ and surrounding sky	70
Intrinsic actinic brilliancy of the brightest portion of the corona	0·031
„ „ „ surrounding sky	0·0007
„ „ „ Full Moon (average)	1·66
„ „ „ surrounding sky	0·000064
„ „ „ sky 1° distant from the Sun (average)	40

Under Prof. Pickering's direction observations were made on the shadow bands; and he seems to have satisfactorily traced them to the irregular refraction of the atmosphere, as influenced by the wind. An electric search-light, or any other strong enough light, is capable of producing similar effects. The paper concludes with some valuable remarks on eclipse observing, which will be read with interest by all those more immediately concerned.

THE TELESCOPE.—In the edition of the 'Encyclopædia Britannica' now being issued there appears under this heading a highly interesting article by Her Majesty's Astronomer at the Cape.

As might be expected from Dr. Gill's most intimate knowledge of the subject, and the great interest he takes in it, the account is a very complete and accurate one, particularly so that portion in which he speaks of the more recently constructed large instruments, many of which he has himself seen. Commencing with a history of the invention of the telescope, in which the claims of the different inventors and improvers are set forth at some length and with great impartiality, the author goes on to describe, as fully as the space at his disposal will allow, all that goes to make up what we know as the telescope, in the many and various forms in which it is used by the astronomer of the present day.

There are chapters on (1) *The Refracting Telescope*, where the conditions that are necessary to be followed to secure a good photographic or a good visual image are considered; we note here that no mention is made of the dialyte form of telescope, a form that is likely, considering the difficulty in procuring large discs of glass, to come to the front in the future: (2) *On Eyepieces*, where besides the many well-known forms, those made by Dr. Schröder are described; (3) *On the Reflecting Telescope* and on most of the many forms in present use; (4) *On the Construction of Object-glasses*; (5) *On the Construction of Specula*, where, we regret to find, there is no mention of the labours of Foucault, who by bringing the manufacture of specula under a system of optical measurement applied during the process of making, in place of the old plan of time and error, which was actually a plan of working in the dark, did as much for the reflecting telescope as was done by the achromatizing of the refractor; (6) *On the Mounting of Telescopes*, where we have, in a comparatively small space, the fullest information on the different ways of mounting both reflecting and refracting telescopes. In order to more distinctly mark the characteristics, Dr. Gill has classified them, and taken one or more of the larger existing telescopes of each class, and fully described them in detail, by the aid of excellent woodcuts; and as nearly all the important telescopes are thus described, this chapter gives practically all that has been done to the time at which it was written (1887).

We note that a design by Sir Howard Grubb is illustrated as for the mounting of the Lick object-glass; but although his design for the rising floor was followed, the actual mounting of the telescope was done by an American firm.

The concluding chapter is on *Driving-clocks*; and we feel, in reading this, that it is a matter for regret that Dr. Gill, who has particularly studied this subject, and is as competent to speak on it as any man living, did not give us some of his own ideas. The needs of photography are such as to bring this question into a prominence it would not otherwise have had; but it may be that the limited space at his disposal prevented this and other subjects being treated in a fuller manner. As it is, it would be difficult to give in fewer words all the interesting matter that we have in his article.

MILAN.—Prof. G. V. Schiaparelli has recently published the results of eleven years' work (1875–1885) on double stars with the 8-in. Merz refractor.

The introduction, which forms the earlier part of the volume, gives an account of the instrument used, with some notes on the sizes of star-discs; a careful investigation of the value of the micrometer-screw, and its temperature coefficient, which is found to be 0·000021 per 1° C.; discussions of the systematic and accidental errors of the observations—notably those depending on the position-angle; a comparison of personal equation with Dembowski, showing that Schiaparelli's distances are slightly larger and his position-angles a little smaller than Dembowski's; and, finally, some interesting "Special notes" on the stars observed. Then follow the measures themselves.

The 465 systems observed are divided into 4 groups:—(1) those in the Dorpat Catalogue; (2) in the Pulkowa Catalogue; (3) those discovered by Burnham; (4) miscellaneous.

The Observatory has now been provided with an 18-in. refractor, which promises to do very good work. It has already revealed the fact that the principal star of ϵ Hydræ is a very close double, magnitudes 4 and 5·5, distance about 0″·2. A few measures made with this instrument in 1886–8 are given in an Appendix.

NOTES.

Diffused Reflexion.—Several investigations have recently been made of the laws governing the diffusion of light from surfaces in directions differently inclined to the normal. Some experiments of Bouguer and Lambert about the beginning of the last century seemed to suggest that the intensity of the light diffused at an angle e with the normal, due to a beam making an angle i with the normal, varied as $\cos e \times \cos i$, and of course inversely as the square of the distance from the diffusing element. If this were the case, whatever be the angle of incidence, most light would always be diffused in the direction normal to the surface; and the rest would spread in symmetrical cones round this normal, the intensity being independent of the relative azimuths of the incident and diffused rays. As the incident ray is made more oblique, the intensity of the light diffused in every direction is diminished in the same ratio. Considering the difficulties in making exact photometric measures, it is not surprising that this law, which holds roughly in a few special cases, remained for a long time without being seriously challenged. But it is not very difficult to show that the law is inaccurate; and recent experiments have shown that the departures from it are so serious, and so numerous, that we must definitely reject it. Prof. H. Seeliger * examined surfaces of clay, milky glass, sandstone, slate, plaster, and porcelain, and sums up his results as follows:—

1. Lambert's law can only be regarded as an approximation, and does not hold for large inclinations of the diffused ray to the normal.

2. Some light is always reflected according to the usual law, even with substances like plaster, which are generally considered to diffuse the whole light which falls on them; so that more light emerges on

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* "Zur Photometrie zerstreut reflectirender Substanzen," Konig. bayer. Akad. *der Wiss. 1888,* Heft 2.

the opposite side of the normal from the incident ray than on the same side. The quantity of the reflected light, however, is in certain cases small, *e. g.* for sandstone; and the author does not consider it impossible that there may be in nature substances for which more light emerges on the same side of the normal as the incident ray. But the light diffused is in any case a function of the relative azimuth of the two rays.

3. Though there are general similarities about the curves, representing diffusion of light by different substances, the discrepancies are so large as to make it improbable that any general law can be formed which would satisfactorily represent the facts.

Herr J. B. Messerschmitt *, also working on much the same lines, comes to nearly the same conclusions. He has compared his results both with Lambert's law as above and with other laws suggested by Lommel and Seeliger; but finds large discordances with few exceptions. A surface of biscuit gave results nearly in accordance with Lambert's law up to an inclination of over 70° for the diffused ray (though according to his diagram it appears as though the diffusion were nearly constant up to this angle?); indeed most substances show curious discrepancies for large inclinations of the rays to the normal. Very rough substances diffuse more light at about 45° to the normal than along the normal itself.

Prof. Seeliger goes on to remark that it appears almost hopeless to attempt to formulate any law for the illumination of planetary discs. For besides the composite nature of their actual surfaces, which will present all the above anomalies at once, we have the further complication of vapour-laden atmospheres. He has, however, suggested for these cases the formula $\frac{\cos i \cos e}{\mu \cos i + \cos e}$ for the intensity of light incident at angle i, and diffused at angle e, μ being a constant: and finds that it fairly represents observations made on Saturn.

JUMPING STARS.—Some time ago a correspondent of 'Nature' (1888, May 31, p. 102) called attention to a "curious apparent motion of the Moon seen in Australia." The description is as follows:—"We saw such a curious phenomenon on Sunday night, about 10.30. Miss C—, Miss H—, and I were sitting in the balcony when we noticed the *Moon apparently dancing up and down.* It was on the wane, so looked so extraordinary. The motion was visible only when she was behind a narrow stratum of cloud and continued at intervals for thirty minutes. I felt quite sea-sick with watching it, and Miss H— was so frightened; she thought there might be an earthquake coming, so went to bed in her clothes to be ready for an emergency." The letter does not seem to have prompted the communication of similar experiences, and possibly the phenomenon is an extremely rare one; but it has been observed more than once in the case of stars, and was called by Humboldt, who seems first to have noticed it, "Sternschwanken." In Astr. Nach. No. 2841, Herr G. D. E. Weyer made a curious observation on the subject; he noticed Antares, on an evening when other stars were scintillating strongly, apparently moving in jerks to the left and then to the right, with intervals of rest. The star was at an altitude of about 4° or 5°, and the apparent motion 2° or 3°. But he found that on directing a telescope to the star, the apparent motion ceased; though on looking again with the naked eye there were still the same jumps, sufficient to have carried the star right out of the field of the telescope. Herr Weyer remarks that he was at the time tired with computation, and that the phenomenon appears to have been subjective in his case, though it agreed well with other accounts of Sternschwanken. In Astr. Nach. Bd. 38, p. 371, is another account by E. Vogel, who saw Venus jumping or dancing in this way on 1853, July 1, from the Tayhonu Mountains; while on August 4, Sirius, and in September, Regulus showed the same irregularities. In writing to Humboldt, Herr Vogel sums up his observations as follows:—

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* "Ueber diffuse Reflexion," Annalen der Physik und Chemie, N. F., Bd. xxxiv., 1888.

1. The phenomenon is best seen in strong twilight, when 2·3 magnitude stars are barely visible, and at an altitude of 5° or 6°.
2. The motion is then parallel to the horizon.
3. The star oscillates rapidly 4 or 5 times in the same direction and then remains 5 or 6 seconds at the further extremity of the arc before returning to mean position.
4. In weak twilight the motion is apparently at an inclination of 45° with the horizon.
5. The dancing does not occur in the absence of twilight or at an altitude of more than 10°.

We should be glad to hear if any of our readers have noticed this phenomenon.

ASTRONOMICAL PHOTOGRAPHY.—We have been requested to give publicity to the following letter, which has been addressed to those astronomers who are interested in astronomical photography, and to ask that any who may not have otherwise received a copy will kindly consider this as addressed to them personally:—

"In accordance with the wish expressed by the Astrophotographic Congress held at Paris last year, we are now occupied with the constitution of a Committee to study the best methods of working and to collect results obtained in celestial photography other than the photographic chart of the heavens (which is in the hands of the Committee of the Congress).

"If you wish to take part in this work we should be glad if you would send to either of us your adhesion.

"Next year, when scientific congresses will be held in Paris, we intend to call together those who have thus expressed their adhesion in order to constitute a Committee and examine the questions that we shall have to consider; we would further ask you to let us know if you would be disposed to take part in this meeting at Paris at a time to be hereafter notified.

(Signed) "J. Janssen,
Président de l'Académie des Sciences,
Directeur de l'Observatoire d'Astronomie physique de Paris.
Meudon (Seine-et-Oise), Paris.
"A. A. Common, F.R.S.,
September 15, 1888. Eaton Rise, Ealing, London."

Longitude Operations.—The Hydrographer has recently been trying some experiments to test the relative capabilities of the sextant and portable transit for obtaining accurate local time.

In the hands of a practised observer the sextant gives very good results by the method of equal altitudes of the Sun; but the personal equation involved is apt to be large, and possibly somewhat changeable. In a recent determination of the longitude Malta—Gibraltar by Lieuts. G. C. Frederick and H. H. Douglas, of H.M.S. 'Sylvia,' the sextant observations differed by $0^{s}\cdot 13$ from those of the portable transit; but there was no exchange of observers, and the determination of personal equation was made some months before. Using the telegraphic determination of the longitude of Malta—Berlin by Auwers and Gill in 1875 the present determination gives for the longitude of a flagstaff at Gibraltar $0^{h}\ 21^{m}\ 25^{s}\cdot 80$ or $5^{\circ}\ 21'\ 27''\cdot 0$ west of Greenwich, that previously adopted from chronometric connections being $0^{h}\ 21^{m}\ 25^{s}\cdot 15$ or $5^{\circ}\ 21'\ 17''\cdot 2$.

In connection with the same sextant experiments some signals were exchanged direct with Greenwich in 1885, giving a longitude of $0^{h}\ 21^{m}\ 23^{s}\cdot 67$ or $5^{\circ}\ 20'\ 55''$; "but the electrical condition of the cable permitted little confidence in this result."

Longitude, Paris—Greenwich.—In No. 138 we mentioned that a redetermination of the Longitude Paris—Greenwich was to be undertaken. Through the courtesy of the Submarine Telegraph Co. and Post Office a telegraph-wire from Montsouris to the Royal Observatory, Greenwich, is granted for the observations from 8^{h} to 12^{h} every night. On the evening of the 24th ult. all was in readiness to commence work, but, unfortunately, clouds prevented observation, so the observers exchanged trial signals only. Messrs. Bassot and Turner are at Paris, Messrs. Defforges and Lewis at Greenwich. It is proposed to take 4 sets of observations (each set to include 3 clear nights, on which observations of 40 time-stars and 160 signals will be made by each observer), and to interchange observers after the first and third sets so as to eliminate personal errors. Clouds have prevented any observations up to the time of going to press.

The U.S. Bureau of Navigation propose sending an expedition to continue the telegraphic determination of secondary meridians of longitude in Central America, early in November.

Comet *c*, 1888.—In Ast. Nach. No. 2855, Dr. Kreutz gives the following Ephemeris for Berlin Midnight:—

1888.	R.A. h m s	Decl. ° ′	Log.	Log.
Sep. 29 ……	15 27 19	+15 27·9	0·1396	0·2611
Oct. 3 ….	15 40 27	12 49·6	·1548	·2772
7 …	15 52 40	10 21·6	·1697	·2937
11 ….	16 4 2	8 4·4	·1842	·3101
15 ….	16 14 43	+ 5 57·4	0·1983	0·3265

Comet *d*, 1888 (Faye).—This comet is slightly increasing in brightness.

Ephemeris for Berlin Midnight (by Dr. Kreutz).

1888.	R.A. h m s	Decl. ° ′	Log *r*.	Log Δ.
Oct. 3 ….	7 12 14	+13 30	0·2530	0·2042
7 ….	7 19 41	12 44	·2553	·1973
11 ….	7 26 43	11 58	·2577	·1904
15 ….	7 33 20	11 11	·2603	·1835
19 ….	7 39 32	10 23	·2631	·1765
23 ….	7 45 17	9 35	·2660	·1694
27 ….	7 50 34	+ 8 47	0·2690	0·1623

Comet *e*, 1888, was discovered by Mr. Barnard, of the Lick Observatory, on Sep. 3, and independently by Mr. Brooks on Sep. 4. The following ephemeris, for Berlin Midnight, is given by Dr. Berberich in Ast. Nach. No. 2858:—

1888.	R.A. h m s	Decl. ° ′	Log *r*.	Log Δ.
Oct. 4 ….	6 35 33	+8 8·4	0·3637	0·3081
8 ….	6 30 2	7 35·9	·3580	·2822
12 ….	6 23 14	6 59·5	·3523	·2550
16 ….	6 14 58	6 18·6	·3466	·2265
20 ….	6 4 57	5 32·4	·3410	·1972
24 ….	5 52 55	+4 40·3	0·3354	0·1672

From the 'Sidereal Messenger' we learn that Dr. H. C. Wilson has been appointed Assistant Professor of Astronomy at Carleton College, and Mr. H. V. Egbert, Assistant at the Washburn Observatory.

Colonel Heaviside, who for over 20 years has been attached to the Indian Trigonometrical Survey and superintended several important operations, has retired.

Prof. Langley gave an address on "the history of the theory of radiant heat" on retiring from the Presidency of the American Association for the Advancement of Science.

We have seen it stated that Prof. Piazzi Smyth's 43 years' work is to be rewarded with a baronetcy.

Miss A. M. Clerke is on a visit to Dr. Gill, at the Cape.

Minor Planet No. 275 has been named Sapientia.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 142. NOVEMBER. 1888.

MEETING OF THE LIVERPOOL ASTRONOMICAL SOCIETY.

Monday, October 8, 1888.

Mr. James Gill, F.R.A.S., in the Chair.

The Minutes of the last proceedings were read and confirmed, and the opening address of the President (Mr. T. G. Elger, F.R.A.S.) was read by Mr. Johnston, F.R.A.S. The address referred to the growing improvement of the Society, and remarked on the wonderful advance it has made as an influential body on astronomical matters. "The true secret of the gratifying state of affairs undoubtedly lies in the fact that the Society encourages the humblest tyro, however crude and elementary his acquaintance with 'the wonders of the heavens' or primitive the optical apparatus at his disposal, to add his mite, small as it may be, to the general sum of astronomical knowledge, and brings him into communication with others of like tastes, who are able and willing to put him in the way of doing useful work. The 'Journal' includes many valuable observations which, but for this encouragement, would never have been elsewhere recorded, even if they had ever been made. The Society may, in short, be regarded as a school for teaching the beginner, by the pleasantest methods and without any very lofty scientific aims, how to observe, and what to observe." The President went on to speak of the educational class, the reports of the different sections, and the contents of the last volume of the Journal. He expressed his indebtedness to Mr. W. Denning for specially valuable illustrated articles on the planets, together with notes on comet-seeking and on meteoric observations; to Mr. Gore for papers on the positions and colours of binary stars and on double stars; and to Mr. K. J. Tarrant for original work in the form of measures of 200 double stars. "To Mr. Monck we owe several important contributions relating to stellar astronomy, eclipses, &c. Our late editor, Mr. Sadler, furnishes two able papers of peculiar interest on 'Stellar Photography,' and there are also articles by Dr. Knott, Rev. J. Johnson, Mr. Lynn, Mr. Stanley Williams, Major Markwick, Mr. W. R. Brooks, and other leading observers." After remarking on these interesting contents, the address goes on to suggest possible improvements by the opening of "Correspondence" and "Notes and Queries" columns; "and though we may have to exercise some patience before we see an illustrated monthly devoted to science comparable as regards paper, printing, and illustrations to 'L'Astronomie' of the French Astronomical Society, we may, at any rate, hopefully look forward to the ultimate attainment of similar excellence in our publication." The President then referred to his own particular subjects—the Moon and the planet Saturn. He spoke sympathetically and encouragingly, and laid down sound lines for future work. In his concluding remarks he referred to the loss the Society had sustained in the resignation of Mr. Davies as secretary, but felt assured that his successor, Mr. W. E. Rowlands, would do his utmost to discharge the duties in connection with that office with equal energy. For his own absence from the Liverpool headquarters the President promised to atone "by devoting myself to the interests of the Society in a wider sphere."

A paper communicated by Prof. E. S. Holden, of the Lick Observatory, on Mars was read by the Secretary, and was followed by an interesting discussion and comments on the beautiful drawings sent for publication by Prof. Holden.

A paper on "Mr. Lockyer's Meteoric Theory," by M. Fergus, excited a short discussion, in which Messrs. Johnston, Gill, and Rowlands took part.

Capt. Jaffrey, Mr. Rowlands, and Mr. Gill volunteered to execute on lithographic paper, for publication in the 'Journal,' the drawings of Mars sent by Prof. Holden, the first-named gentleman undertaking half of the number.

A vote of thanks was accorded to the President for his interesting address, and to Mr. Johnston.

Photographs of Nebulæ.

It is not many years since the idea of photographing such objects as nebulæ would have been considered impossible, more particularly by those who know most about the subject. Sir John Herschel, in the correspondence which took place about the making of a large telescope for the Southern Hemisphere, which resulted in the 4-foot reflector at Melbourne, expressed his opinion that photography might be used for getting the positions of stars near nebulæ, in order to assist their delineation by hand; but even he, whose knowledge of the nebulæ and of the powers of photography was so great, does not seem to have indulged in the hope that they would ever be photographed directly, possibly because he knew the great difficulties involved; indeed many years before, one who knew less about nebulæ or photography (at that time quite a new art) had, with the confidence of half-knowledge, expressed quite a different opinion. This man was Dr. Dick (whose charming works are not held in such repute as they deserve, or, at any rate, do not seem to be read as they ought to be), who said, as early as 1845, just after the advent of daguerreotype:—"Nor is it impossible that the planets Mars, Venus, Jupiter, and Saturn may be delineated in this way, and objects discovered which cannot be discerned by means of the telescope. It might be perhaps considered as beyond the bounds of probability to expect that even distant *nebulæ* might thus be fixed and a delineation of their objects produced, which shall be capable of being magnified by microscope. But we ought to consider that the art is yet only in its infancy—that plates of a more delicate nature than those hitherto used may yet be prepared, and that other properties of light may yet be discovered which shall facilitate such designs. For we ought now to see no boundaries to the discoveries of science, and to the practical applications of scientific discovery which genius and art may accomplish." This was a long look ahead at the time; but all that Dr. Dick imagined as possible has since been done.

What may be done, or, rather, what will be done as better processes come to the front it is impossible to say, nor do I wish to discuss this aspect of the question in this short note, but rather to deal with what has been shown to be quite possible, and to make some remarks on what has been recently done, particularly by the MM. Henry and Herr von Gothard; and my excuse for doing so is that so little seems to be known as to what can and what cannot be done of a useful character in this department of astronomical photography, that discussion of the subject seems to be necessary. The photographing of nebulæ is remarkable as being almost the only actually modern achievement of photography; all else we can do now could be done in some form or other soon after the first application of photography to astronomy. It is true that the pictures of stars could not be taken as quickly then as they can now; but stars could be taken; while all attempts to photograph comets or nebulæ failed or were not persevered in through the apparently hopeless nature of the attempt. We are now enabled to get pictures of the nebulæ that are certainly far superior to any drawings, and, what is of immense importance, with instruments of ordinary or moderate size.

In the practical work of photographing the nebulæ, whose light, as a rule, is so very feeble, the aim has been to collect as much as possible on the plate; and in order to do this the telescope with the smallest ratio of focal length to aperture has been used, as giving the greatest amount of light on the least surface; and if the image produced on the sensitive film were made up of particles so small as not to affect the shape of the image, it would matter little what the size of that aperture was. But, as is well known, the particles of silver that gather together to form the image have a sensible size, and by an appearance similar to stippling or mezzotint produce a picture. When this picture is small compared to the particles of silver that form it, detail is lost; and thus the absolute scale of the picture is a matter of importance.

How far processes will be improved in the direction of reducing the size of the silver particles and giving a perfection to the image that at present it has not, it is not possible to say, though there is a promise of improvement in this direction without loss of sensitiveness.

Pending such a solution of the difficulty, it is of interest to consider at the present time what size of image can be utilized—in other words, what is the least aperture that can be usefully employed in taking photographs of the nebulæ. As in all such discussions one ounce of practice is worth a great weight of theory, it is most satisfactory to appeal at once to fact and experience.

Dr. H. C. Vogel, of Potsdam, gives, in No. 2854 of the 'Astronomische Nachrichten,' a very important note on the recent work of Herr von Gothard in photographing nebulæ with a 10-in. reflector, with some drawings that show in a most unmistakable manner with what effect telescopes of this aperture can be used.

In this paper Dr. Vogel expresses an opinion that the photographs of Herr von Gothard taken with this comparatively small instrument show results which far surpass any obtained by eye observation with the largest telescope. He mentions the fact that Herr von Gothard some few years ago discovered by photography a small star within the ring-nebula in Lyra, only a few uncertain observations of this star having been previously made, and describes its continued appearance on the photographic plate though invisible in the largest refractors; although I think that there is some slight mistake here, as the star has always been visible with sufficient optical power, still the fact of getting this star so plainly pictured is not less important. We are not, however, concerned with this, but with the work illustrated and described by Dr. Vogel. Six well-known nebulæ are illustrated on a larger scale by drawings made from the photographs, and these can, in most cases, be compared with existing drawings, and the value of those photographic pictures properly estimated. As it happens, I can speak with some degree of confidence as to one of these.

This, the spiral nebula in Canes Venatici (G. Catalogue 3572), is a nebula that is most peculiarly fitted to deceive the eye by the shape of the outlying parts; hence the great difference in the various published drawings.

In the course of 1883 I took, with about thirty minutes exposure, a photograph of this nebula with a 3-foot aperture, and got a picture somewhat similar to that obtained by Herr von Gothard, *but with less detail*; I made at the time an examination of most of the drawings mentioned by Dr. Vogel, and can quite bear him out in all he says as to the different ways in which they show this nebula. A point of great interest comes out on examining these photographs, mentioned by Dr. Vogel—that is, the peculiar aspect of the knotted parts of the nebula, that look almost like stars, but have quite a different character; but a description of a photograph is almost impossible, and to compare the drawings with it is the only proper way to get a correct idea of the respective values.

During the past year MM. Henry at Paris have taken a picture of the Pleiades that brings some most important features to light, showing that this group of stars is in a mass of nebula. This has been suspected by many observers from time to time, but no one had been able to localize any of the brighter parts that have since been shown by photography, except perhaps an observation made with a 3-foot in 1880, that showed two or three of the brighter portions that have since been shown so beautifully on the photographs of the Brothers Henry; on these latter plates not only were these and other masses more or less intense practically discovered, but the positions of exceedingly small stars are given in a way that cannot be attempted by observation or measurement in the ordinary methods—stars that are far too faint to be visible except in the largest telescopes, and even with these not susceptible of accurate measurement. In this particular case I had some years ago observed several exceedingly faint stars near Alcyone that were just visible in a 3-foot reflector, and all of these, with one exception, are shown on the photograph referred to, taken with a photographic telescope of 13 inches aperture.

Similar photographs of the Pleiades have also been taken by Mr. Roberts, near Liverpool, that show almost as much as the photographs of the MM. Henry; but if one can judge by a comparison of the beautiful engraving of the photographs taken at Paris, with the paper prints of those taken by Mr. Roberts, with less distinctness, particularly as to the peculiar line of light, or rather lines of light, so well marked in the Paris photographs. The work of Mr. Roberts was done with an 18-in. silvered-glass reflector, and with a shorter exposure than in the other cases.

In the case of the photographs of Herr von Gothard, the exposure was necessarily extended to over two hours, and in the case of the Henrys' photograph to four hours; but length of exposure is of minor importance when we know that once the photograph is secured it is an actual representation that can be afterwards dealt with at leisure and measured, giving then all the results that can be obtained by eye with an instrument far more powerful than that employed. The use of large telescopes will always be in such work of great value, no matter how processes may be improved upon, as the advantage will still be with them, when minute details of structure possibly hitherto unsuspected will be brought out; but even at the present time we can see from the work that has already been done that we need not look to these large telescopes alone; and as the number of known nebulæ is already so great, there is an amount of work to be done that will occupy any possible number of observers for many years if only in getting impressions on such a scale as those obtained by Herr von Gothard: improvement may come afterwards; but improvement can only follow actual work done.

The work of Herr von Gothard is a solid contribution to our practical knowledge of nebular photography, and goes a long way towards settling the capabilities of a certain aperture. Many other questions remain, and it is to be hoped that observers, encouraged by the remarkable success of Herr von Gothard, will do what they can by actual experiment with the means at their disposal, whatever those means are, to throw more light on this most interesting subject.

How far the great expense of large instruments and the increased difficulty of working them are compensated by the larger scale on which the photographs will be obtained; how far it will be advisable to increase the focal length of moderate apertures to get a larger scale at the expense of longer exposure; to what extent it is advisable to go in the other direction and increase the aperture beyond what is usual even now; how much will be gained by the use of orthochromatic plates or of special plates for special nebulæ; what is the best kind of clock to employ and the best means of giving intermittent exposures,—all these and many other questions will come up for discussion next year when those interested will meet for that purpose. A good deal is becoming known, it is true; but many no doubt thought that 10 inches was much too small an aperture to attempt serious work, and in the same way there may be wrong ideas as to the insufficiency of their means preventing men joining in this work. With Herr von Gothard's example before them they should accept nothing till it is put to the proof of experiment, and by this means we should soon have a fair knowledge not only of what can be done, but of (what is almost equally important if it has been proved) what cannot be done.

A. A. Common.

American Telescopes *.

Under the above title Prof. D. P. Todd gives a most interesting account of the manufacture of telescopes in the United States, and the discoveries made with them since 1826. In this year Amasa

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* Appendix, Vol. xxiii. 'Encyclopædia Britannica,' American Reprint. J. M. Stoddart and Co., publishers.

Holcomb, a surveyor, began to grind and polish lenses for surveying-instruments, and failing to obtain large disks for refractors, tried Herschelian reflectors, of which he constructed 30. Twelve years later Messrs. Mason and H. L. Smith made the then largest reflector in America (12-in. aperture), which Mason used with great success in observing nebulæ. In 1858 Mr. Rutherfurd figured an 11¼-inch reflector; 5 years later he began the construction of the first photographic objective corrected solely with reference to the photographic rays; and in 1869 he applied a photographic corrector to a 13-inch telescope. The next constructor mentioned is Dr. Henry Draper, who in 1858 made a 15-inch speculum, and between 1866 and 1870 a 28-inch silver-on-glass mirror. Mr. Brashear is described as "the leading maker of reflecting-telescopes in America," he having "built" about 100 with mirrors ranging from 5½ to 15½ inches in diameter, in addition to several refractors up to 16 inches. He has recently established works for experiments on optical glass, which it has been found difficult to procure in America. The Lick spectroscope, too, is Mr. Brashear's work. Henry Fitz, of New York, is credited with being the first American who obtained special distinction for the manufacture of refractors; he constructed 30 with object-glasses varying from 6 to 16 inches in diameter. Reference is made to the work of Spencer (1853), Clacey (1875), and several makers of small instruments. Messrs. Lyman, Brooks, Davis, Gundlach, and Hastings are mentioned as "fine mechanicians interested in astronomical work," and an account of their instruments is given.

Of the work of the late Alvan Clark, Prof. Todd says:—

"Of all makers of telescopes the late Alvan Clark was the most celebrated. Known as a portrait painter in Boston in 1844, accident drew his attention to the construction of a small reflector, the success of which was such as to lead to his making refractors. By 1846 he was established in the business of telescope-making with his two sons, Alvan G. Clark and George B. Clark, who survive their father (who died in 1887), and continue the work of the firm at Cambridgeport, Mass. Clark's first refractors were introduced to astronomers through the Rev. W. R. Dawes, of England, who purchased five of them. The Clarks were four times called upon to construct 'a telescope more powerful than any now in existence,' first in 1860, one of 18½ inches aperture, now at Chicago (exceeding the then largest objective by 3½ inches); second, in 1870 under contract with the U. S. Government, for a refractor of 26 inches aperture (the largest at that time was of one inch less diameter); third, in 1879 with the Russian Government, for a 30-inch objective (3 inches greater than the Vienna telescope); and fourth, in 1880 with the trustees of the Lick estate, for the 36-inch telescope completed in 1887 for the Lick Observatory. In this objective the lenses are set 6·5 inches apart, in a cast-iron cell faced with strips of silver where the glasses rest against it.

The weight of the objective, cell and all, is about one third of a ton. Its cost was $53,000. An additional lens, or photographic corrector, of 33 inches aperture was provided at a cost of $13,000.

"Alvan Clark was in 1867 awarded the Rumford medal of the American Academy of Arts and Sciences for the perfection of his optical surfaces. The completion of the 30-inch Russian object-glass brought the Clarks the signal honour of the golden medal of the Empire, conferred for the first time by Alexander III. The Clark objectives are more perfect than the atmosphere in which they can generally be used; and when fine conditions are present they will bear magnifying-powers of 100 diameters for each inch of aperture, and separate the components of double stars up to the limit given by Dawes's empiric formula (Mem. R. A. S. xxxv. p. 159).

"The Clarks have built no less than eleven instruments of the type known as the 'horizontal photoheliograph,' a device originating with the late Professor Winlock in 1869. These have photographically corrected objectives, of 5-inch aperture and about 40 feet focus, mounted horizontally in the meridian, the rays from the Sun or other celestial object being constantly reflected upon the objective by 7-inch plane mirrors, appropriately mounted and moved by clockwork. One of these instruments belongs to the Harvard College Observatory, eight to the U. S. Naval Observatory, and one each at Princeton College, New Jersey, and at the Lick Observatory, Cal. Between the apertures of 6 inches and 9 inches inclusive the Clarks have made over 50 objectives, and have also made the mountings for nearly all of them."

Makers of eyepieces, micrometers, and mountings, with the specialties of each, are next dealt with, and of the mountings Prof. Todd says:—

"The modern American mountings provoke little unfavourable criticism. In some classes of observation difficulties arise from their lack of rigidity, both as to the axes themselves and the ironwork supporting their bearings. But troubles quite as frequently come from lack of stability of the pier and foundations; and too great care cannot be expended upon the founding and construction of these piers. In general our mountings are surpassed in rigidity by those of like dimension from the best English and Irish shops. If the American telescope is compared with those of foreign makers, in point of subsidiary apparatus for its convenient working, we find a constantly growing disposition to consult the wants of the astronomer, and to profit by his suggestions."

Notes on the makers of transit instruments and meridian circles and Mr. Chandler's Almucantar follow.

The works of Bond, Bartlett and Draper in connection with photography are summarized, and the interesting article concludes with a brief review of the most striking and important results reached with American telescopes, in which Prof. Todd says:—

"In January 1862, Mr. Alvan G. Clark, in trials of the 18½-inch

Chicago refractor, then just completed, made the discovery of a faint companion to Sirius, which has since moved approximately in the orbit assigned to it by theory.

"Burnham has made more than 1000 discoveries of new double stars, chiefly with a 6-inch Clark telescope formerly his own, but now belonging to the Washburn Observatory. His chief paper on double stars is an extensive one (Mem. R. A. S. xliv.).

"More than 100 small planets have been discovered with telescopes of American make, Dr. Peters, of Clinton, heading the list with 47, Palisa, of Vienna, coming next with 38, while Watson, of Ann Arbor, found 22, one of them with the 5-inch equatorial of the U. S. Transit-of-Venus-Expedition to China, 1874, of which he was the astronomer in chief. The Lalande medal of the Paris Academy of Sciences has been five times awarded to American discoverers of small planets, lastly to Peters, in 1879, whose work is worthy of more extended notice than we have space for, on account of the elaborate star-charts of the zodiacal regions which he is now publishing.

"In 1877 Pickering devised the new photometric method of observing the eclipses of Jupiter's satellites, whereby the varying light of the waxing or waning body is repeatedly compared by means of a photometer with an uneclipsed satellite near it. Nearly 400 eclipses have already been thus observed; and, in 1890, on the planet's completing an entire revolution about the Sun, the discussion of the whole series will form a most important contribution to our knowledge of the distance of the Sun.

"Hall's observations of Hyperion, the seventh satellite of Saturn, brought to light a disturbance in its motion which appeared as a retrograde movement of the line of apsides of its orbit. Subsequently Newcomb developed the mathematical theory of this inequality and showed it to be due to the excessive perturbing action of the large inner satellite, Titan. Four times the motion of Hyperion is almost an exact equivalent of three times that of Titan; and the motion of Hyperion thus presents an entirely new case in celestial mechanics, and to which the ordinary mathematical theory is entirely inapplicable.

"The first photographic record of an unknown celestial object was made by Pickering, the 3rd November, 1885, when a nebulous mass was seen to surround the star Maia of the Pleiades, on a plate exposed on that date; but failing to recognize its true character at that time, the first published result came from precisely similar work two weeks later by the Brothers Henry, of Paris. This singular object was first discovered optically a short time afterwards by Struve with the 30-inch Clark refractor at Pulkowa. The epoch-making researches of Huggins in stellar and nebular spectroscopy with an 8-inch Clark refractor are well known.

"Of all the heavenly bodies, the planet Mars now holds the most signal position, by reason of the peculiar circumstances precedent to and accompanying the discovery of its satellites, two in number, both found by Professor Asaph Hall at the opposition of the planet in 1877." J. P.

The Meteorology of the Summer of 1888.

In the present note we propose to consider the meteorology of the four months, June to September, which practically may be said to form our summer period—one the greater portion of which in the present year has been remarkable for low temperature, want of sunshine and corresponding large preponderance of cloud, and excessive rain. Some statistics in regard thereto, founded on the observations made at the Royal Observatory, Greenwich, may therefore at the present time prove interesting.

Firstly as regards atmospheric pressure. The mean reading of the barometer, not reduced to sea-level (elevation 159 feet), was in June 29·75 inches, in July 29·60 inches, or about 0·2 inch below the average, in August 29·83 inches, and in September 29·97 inches, or nearly 0·2 inch above the average.

Air temperature has been on the average low in all months, the mean temperature having been in June 58°·3, in July 57°·9, in August 59°·1, and in September 55°·7, below the average respectively by 0°·6, 4°·3, 2°·4, and 1°·4, the defect in July being one of marked character. The temperature during June was very unequal—high at first, low during the middle of the month, June 18, 19, and 20 being especially cold, and variable afterwards. In July the temperature was below the average on every day of the month, and indeed was so from June 27 until August 6 inclusive, a period of 41 days, or nearly six weeks. July 11 and 12 were days of remarkable cold, the maximum temperature reached on those days being only 55° and 54° respectively, the mean temperatures, 48° and 50°, being 15° and 13° below the average, corresponding to temperatures of about the end of October or beginning of November. The only years since 1841 in which the July temperature was correspondingly defective were 1841 = 57°·8, 1860 = 57°·6, and 1879 = 58°·1. The highest temperature reached in July was 74°·0. There have been a few years since 1841 in which the maximum temperature of July did not exceed 80°, but no one in which it failed to reach a higher point than in the last July. August was generally a cold month. The first half of September was cold, but afterwards a better condition of things prevailed.

A remarkable defect of temperature has now been experienced for a considerable period. Since 1886 December the mean temperature of every month has been below the average, excepting the months of June, July, and August of 1887; that is, in 19 out of 22 consecutive months the mean temperature has at best only just touched the average, whilst in some months the defect has been very marked, as in March and October of 1887, and in February, March, April, and July of 1888.

The duration of bright sunshine in June was 131 hours, in July 96 hours, in August 146 hours, and in September 122 hours; below the average in June and July, especially in the latter month, about an average in August, and somewhat above in September. Taking June and July together the number of hours of sunshine in 1887 were 507, and in this year only 227, the average being about 350. In June and July of 1887 the percentages of cloud were only 50 and 46 respectively, but in 1888 they were 78 and 85. Only in two years since 1818 has an amount of cloud smaller than that of July 1887 been recorded, namely in 1825=43, and in 1835=33, and only once has that of July 1888 been equalled, namely in 1879, when it was 85.

The rainfall in June was 3·35 inches, in July 6·75 inches, in August 3·73 inches, and in September 0·73 inches, excepting the last all above the average. Three large falls occurred, on June 26=1·70 inches, on July 30=2·49 inches, and on August 1=1·44 inches, in all cases accompanied by thunder-storms. On July 30 the large amount (for Greenwich) of 2 inches fell in the space of three hours, causing floods and doing great damage to property.

The contrast between the months of June and July of last year and this year is somewhat remarkable. Something has been already said in regard to sunshine. But it may be added that whereas in June and July of 1887 there were 43 days on which more than 6 hours of sunshine were registered, in the same months of this year there were but 15 of such days. Correspondingly the days without sunshine were in 1887 but 3, whilst in 1888 there were 12. In June and July of last year the temperature rose above 80° on 21 days, and in this year on 3 days only. The mean temperature of July 1887 was just as much in excess as that of 1888 was in defect, the numbers being 66°·5 and 57°·9, a difference of 8°·6. In June and July of last year the total rainfall was 2·52 inches, and in this year 10·10 inches, the average being 4·37 inches. In no year since 1841 has the amount 10·10 inches been equalled, the greatest amounts collected in these months having been 8·23 inches in 1853, 8·60 inches in 1860, and 8·01 inches in 1879. In June and July 1887 rain fell on 13 days, and in 1888 on 41 days, there having been in July alone of the latter year 26 days with rain out of 31.

The character of the month of July 1888 has thus been especially exceptional—low atmospheric pressure, low temperature, little sunshine, much cloud, and much rain. To a certain extent these things necessarily go together, but it is not always that extreme in one element is accompanied by so extreme a condition in all, or that the same month in two successive years should have characteristics so widely different. We can only hope that the late July was an extreme amongst extremes, and that we may not soon look upon its like again. W. E.

Meteor Notes.

THE month of November is distinguished for the numerous fireballs visible, as well as for the Leonid and Andromede meteor-swarms which periodically recur at this time. But though many fireballs are seen, very few meet with full and accurate record; hence it is rarely found possible to compute their real paths in the atmosphere. In Nov. 1887 fireballs appeared on the 13th, 15th, 17th, 19th, and 21st; but the accounts were too indefinite to be of service. The fortunate observers of phenomena of this kind will be rendering useful aid to this branch by noting the directions and positions of bright meteors with reference to stars lying near their paths, and by estimating their apparent directions of flight. Showers in Taurus and that region usually furnish many large meteors in November.

On the mornings of Nov. 13, 14, and 15, after moonset, the eastern sky should be watched for traces of the swift streak-leaving meteors from the great shower in Leo, and again, before moonrise on the nights of the 26th and 27th, for the slow, trained meteors from near γ Andromedæ. We are not near a maximum in either case; but it is most desirable to secure annual observations, so as to ascertain the relative richness of the various sections of the meteor-orbits in which the Earth becomes immersed. The Leonid stream is continuous, though by no means rich in places far from the parent comet; but the Andromedes have been but slightly observed except during their more brilliant displays, and it is not certain they are dispersed along the complete ellipse forming the orbit of Biela's comet.

Recent Observations.—Mr. D. Booth, at Leeds, observed a large number of shooting-stars between Oct. 1 and 13, including 60 on the night of Oct. 3 alone, and has deduced the following radiant-points:—

Ref. No.	No. and appearance of meteors.	Radiant.				Previous observations.
		°	°	°	°	
1...	8, slow.	3	+ 5	2	+ 2	Sep. 24, '75. Fireball. A. S. H.
2...	15, quick.	26½	+15½	23	+17	Oct. 5–7, '85. W. F. D.
3...	11, rather quick.	48	+42	47	+44	Oct. 17–24, '87. W. F. D.
4...	6, quick.	58	+12	58	+11	Oct. 13, '69. Tupman.
5...	14, quick.	77	+56	78	+57	Oct. 14–15, '79. W. F. D.
6...	6, quick, with streaks.	90	+13½	88	+13	Oct. 13, '69. Tupman.
7...	7, quick.	305	+72	310	+77	Oct. 3–4, '77. W. F. D.

No. 6 is obviously an early display of Orionids. They were seen by Mr. Booth on Oct. 13, 14^h to 17^h, and the radiant, as he determined it, is very near that found by Col. Tupman for the same shower and on the same date, 19 years ago. In 1887, Oct. 11–14, I fixed the centre at 91° +17° from 5 meteors. This well-known system certainly continues visible between Oct. 9 and 29; but I have never detected any changes in the place of its radiant similar to those which affect the Perseids. In addition to the Orionids, Mr. Booth saw active showers in Aries, near β Persei and near the head of Auriga, and these severally appear to have been observed on some prior occasions.

Large Meteors.

1888, Oct. 3, $8^h\ 39^m$, = ♀. It lit up the sky in the region of Camelopardalus, and left a bright streak enduring 30 secs. Path 68° +70° to 60° +62°.—*D. Booth, Leeds.*

Oct. 3, $9^h\ 45^m$, = ♃. From near γ Persei to a little beyond ι Arietis. Duration fully 3 seconds.—*Rev. W. R. Waugh, Portland.*

Oct. 11, $7^h\ 35^m$, > ♀. Path from 320° +40° to 285° +35°, passing almost across γ Cygni. Duration 1 sec., very swift. Head very large and bright. Train rather thin.—*Rev. W. R. Waugh, Portland.*

Oct. 12, $16^h\ 41^m$, = ♃. Swift, streak. Path from 45° +82° to 296° +61°. Meteors very numerous Oct. 13 A.M., including several Orionids. 3 faint telescopic meteors seen between $16^h\ 41^m$ and $16^h\ 51^m$.—*W. F. D., Bristol.*

Oct. 14, $8^h\ 19^m$, = ♀. Path from 3° *s.p.* ε Pegasi to 2° *n.f.* ι Ceti and disappeared 3° after. Quick. Last 3° or 4° of course only well seen. Bright train.—*T. W. Backhouse, Sunderland.*

Mr. Booth's letter ('Observatory,' Oct. 1888, pp. 379–380) is interesting. His data fully corroborate the displacement in the great Perseid radiant-point. This striking feature of the shower has often been noticed here since I first detected it in 1877.

Bristol, 1888, Oct. 17. W. F. Denning.

Selenographical Notes.

J. Herschel.—This large formation, some 80 miles in length and of about equal breadth, adjoining Anaximander on the west, was unnamed, and to a great extent unregarded, till Birt gave it the designation by which it is now known to selenographers. Unfortunately it is in too close proximity to the north-eastern limb for an exhaustive scrutiny, even under the best conditions of libration; but, in spite of this, it deserves far more attention than it has hitherto received. It is in many respects an important formation, to which neither Mädler nor Neison do justice. If the observer chooses a time when the east longitude of the morning terminator is about 39°, he will find that the bright western border of J. Herschel approximately coincides with it, that the interior of the brilliant ring-plain Horrebow, situated near its most southerly extension, is in shadow, and its hour-glass-shaped wall only partially illuminated. At this stage he will not fail to note what appears to be a wide dark gap in the western flank of this object, and a less pronounced opening on the opposite side. If definition is good and the power employed a high one, the former will be resolved into a brilliant little crater on the wall, filled with shadow, which Schmidt alone among selenographers portrays. At $8^h\ 15^m$ on October 15 I saw this object very satisfactorily with a power of 284 on my 8½ Calver-reflector, and in addition a second smaller and less bright crater on the extreme northern end of the wall. The peculiar hour-glass form of Horrebow is remarkable. Schwabe in his MS. collection of lunar notes and drawings, now in the Library of the R.A.S., compares the formation, under date March 20, 1826, to "a double crater" on the edge of J. Herschel. At times under a low power it certainly has this appearance. The constriction in the outline of the border occurs near the site of the crater on the west wall and of the gap on the opposite side. It seems probable that at some former epoch the formation really consisted of two contiguous rings, which have subsequently merged into one through the collapse of that portion of the wall common to both.

Under the conditions just supposed to obtain, little can be seen of J. Herschel except the massive but apparently slightly discontinuous wall which separates it from the Mare Frigoris, a narrow strip of floor just in sunlight near the centre, and a very brilliant but small crater shining with dazzling whiteness in the midst of the shadow of the wall. This latter is the principal object of its class within the circumvallation. At a latter stage, when the morning terminator is a little beyond east long. 40°, the eastern wall is partly in sunlight and the true character of the formation apparent. It is seen to be surrounded by mountains of considerable dimensions and is clearly something more worthy of regard than Neison's description of it as a "considerable depression between Anaximander and Horrebow" would lead one to expect. Its individuality is, in fact, far more distinctly marked than that of many other objects of a similar type which owe their notoriety to their more favourable position on the disc. It is very interesting to watch the progress of sunrise on the interior from the time when the light first impinges on the strip of floor just mentioned till the inner slope of the eastern wall is illuminated. At first, owing to the multitude of craterlets (large and small), low ridges, and other objects therein, the general effect is somewhat confusing; but when the whole of the floor is in sunlight, in addition to these features, it is seen to be covered with a number of narrow parallel grooves of irregular width, extending in a meridional direction from wall to wall. Occasionally, during glimpses of best definition and with high powers, the number of these grooves is immensely increased, the greater part of them being of extreme fineness, resembling fine dark ruled lines, and reminding one of the more delicate striæ on the face of a glaciated rock or boulder. I know of no similar appearances elsewhere on the Moon; I am led to believe that the more delicate of these curious markings are only visible on rare occasions, as I have only seen them perfectly twice during the past six years, though the coarser grooves have been frequently very obvious.

Schmidt appears to have bestowed no little care on this formation, judging from the amount of detail shown in his map (Sect. xvi.). Adjoining the large central crater on the south, he draws a very remarkable forked ridge which is well worth examining. I have seen it several times, much as he shows it. None of the grooves or striations are portrayed, though he draws many longitudinal ridges. The crater on the west wall of Horrebow appears, but not that seen by me on the north wall on October 15.

It is difficult to advance a satisfactory explanation of the striations. They can hardly represent the shadows of ridges, as they are too black, too fine, and too sharply defined. They seem to partake more of the character of clefts, though here again their closeness to each other and strict parallelism seems to remove them from the category of ordinary lunar features of this type.

Thos. Gwyn Elger.

Kempston, Beds, 1888, October 18.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Occultation of a Star in Pisces.

Gentlemen,—

On turning my telescope to the Moon on the evening of February 16 last, I accidentally observed the occultation of a reddish 7-mag. star, to identify which all efforts so far have proved unavailing. The time by sextant was $6^h\ 26^m\ 40^s$, and the immersion took place at an angle of 130° (by estimation), to the right of the vertex as seen in an inverting telescope. The position of the star would be, approximately, R.A. $1^h\ 28^m$, Dec. 3° 52′ N. No such star is marked about this point in the larger maps of the S.D.U.K. or in any other atlas that I have come across. Only in Bode's maps (1780) there is a small star marked near μ Piscium, whose position is then about $1^h\ 20^m$ and Dec. 4¼° N. At the time R Piscium would be not very far from a minimum. Perhaps some variable-star observer may throw a little light on the matter.

Faithfully yours,

S. J. Johnson.

Melplash Vicarage, Dorset,
1888, Oct. 12.

Sir William Herschel's Observations of Volcanoes in the Moon.

Gentlemen,—

I have been much interested in the correspondence on the above subject in Nos. 139 *et seq.*, and in connection therewith should like to put on record an observation of my own made many years ago.

The late Mr. Webb ('Celestial Objects,' edition 4, p. 108) mentions having seen La Hire "on the terminator, $2^d\ 7^h$ after first quarter, the brightest object in sight, and radiating as described by Schröter." Mr. Webb proceeds: "At another time I noticed a similar hill, about $\frac{1}{4}$ of the distance from Lambert to Timocharis, glittering on the terminator like a star with rays." In a note he adds: "seen by Hunt, 1862." The date should be 1863. My memorandum written at the time of observation runs thus:—"1863, Nov. 19, about $9^h\ 15^m$ G.M.T. Saw this phenomenon very beautifully. The hill shone out very splendidly, accompanied by brilliant flashes. It appeared as if it were very difficult to focus; but having very carefully put it into focus, I have a conviction that I saw no more flashes; but a sudden clouding over prevented my satisfying myself on this point. It certainly very much surpassed in brilliancy anything I have ever seen on the Moon's surface." I saw this at Edgbaston with an equatoreal of 4 inches aperture, by Troughton and Simms, the objective being exceptionally good. I have a most vivid recollection of its extraordinary brilliancy, and have never seen anything like it, before or since.

Unfortunately I did not note the position of the terminator, but on referring to the 'Nautical Almanac,' 1863, I find the first quarter was on Nov. 17 at about 15^h G.M.T.; consequently nearly two days afterwards the terminator must have been somewhere in the immediate neighbourhood. Yours faithfully,

Hopefield, West Dulwich, Oct. 16. GEORGE HUNT.

Jumping Stars.

GENTLEMEN,—

I have read with interest the note on the above subject which appeared in No. 141. The phenomenon referred to is not a common one, but I have witnessed it probably seven or eight times in the last twenty-five years, and always under very similar conditions. These conditions chiefly are:—(1) That the object observed should be a bright one in a fairly clear sky; (2) that it should be observed at a low altitude, say 5° to 8°; (3) that the observation should be made within about an hour of sunset; and (4) that the line of sight should cross a district having a temperature above that of the air.

The most striking example of the phenomenon which I have noticed was in March, 1887, when Jupiter was seen at an altitude of about 7° in a state of apparently violent motion. The oscillations were both horizontal and vertical, and appeared as if extending through an arc of $\frac{1}{2}$ to $\frac{3}{4}$ degree. In this case the third condition above mentioned was not satisfied, the sun having set some four hours or more prior to the observation, but there had been a very marked fall of temperature after sundown, a fairly mild day being followed by a very cold night. I have also seen

Sirius similarly agitated on two occasions and Capella also, but to a much less marked extent. It is worthy of note that from here I have never seen the effect strikingly produced except on bright stars near the south-east horizon, in which case the line of sight would cross a large portion of London, and hence pass over a large mass of more or less heated buildings.

I have always attributed the phenomenon to the same cause as produces the shimmer seen when viewing terrestrial objects across a tract of heated sand, namely a current of heated air rising between the observer and the object. I may add that on one occasion when Jupiter showed the apparent motion referred to I examined the planet with a telescope, using a low power giving a field about 40' in diameter. The image was very unsteady, oscillating apparently some 8' or 10', and the definition strongly "boiling"; but the motion was much less striking than that apparent to the naked eye.

Yours faithfully,

Kensington, 1888, Oct. 3. W. H. Maw.

Alleged Eclipse of the Sun at the Battle of Crecy.

Gentlemen,—

It is not often that we are able to follow a supposed astronomical phenomenon which never really took place in the process of manufacture. Such, however, is the case with regard to an alleged eclipse of the Sun which is stated by many historians to have taken place on the morning of the battle of Crecy, August 26, 1346. Holinshed speaks of "an eclipse," Lingard, Keightley, and Barnes call it a small or partial eclipse. But the 'Pictorial History of England' (commonly called Knight's, from the publisher) states that there was a "fearful eclipse of the Sun." As a matter of fact no eclipse of the Sun was visible in Europe that year. At the time of the battle the Moon had just entered on her first quarter, and she was partially eclipsed six days afterwards, on the 1st of September.

The mistake is founded on a misunderstanding of a passage in Froissart and was fully explained by Sir G. Cornewall Lewis in an article contributed to 'Notes and Queries' and published in the number for April 4, 1863, nine days only before the lamented writer's death. In the passage in question, Froissart, after mentioning the heavy rain which did so much injury to the bow-strings of the Genoese, adds that there took place also "un tonnerre et un esclistre moult grand et moult horrible." With regard to the word *esclistre*, Buchon in his edition of Froissart remarks:—"Plusieurs manuscrits portent *esclipse*; mais il est certain qu'il n'y eut point d'éclipse le 26 août, jour de la bataille de Crécy." He suggests, therefore, that the word signifies "l'obscurité qui précède ordinairement un grand orage." Bourchier, the English translator (at the suggestion of Henry VIII.) of Froissart, rendered the sentence "and a clyps with a terryble thonder"; and Johnes, who published his new translation of Froissart in 1803 (not having explained, as Buchon did afterwards, the question whether there could have been an eclipse), also takes it as such and uses the expression which has been adopted in Knight's "Pictorial History" and is quoted above. One would have thought, however, that a consideration of the whole passage would, of itself, have led to a suspicion that no eclipse, in the astronomical sense of the word, is intended. To Sir G. Cornewall Lewis, however, is due, as I said before, the complete elucidation of the matter. He points out that the old French word *esclistre* signified lightning. Thus Roquefort explains *esclistres* as *éclaires*. It may be remarked that "moult" is also an obsolete French word, of which Littré regrets the loss, as better representing than *beaucoup* the Latin *multum*, from which it is derived. A very great and very horrible storm of thunder and lightning we can understand; a fearful eclipse of the Sun is not so intelligible; but it is certain that no eclipse, great or small, took place on the day of the battle of Crecy.

Yours faithfully,

Blackheath, 1888, Oct. 6. W. T. LYNN.

Motion of the Red Spot on Jupiter.

GENTLEMEN,—

During the past opposition of Jupiter I saw this marking on few occasions. The position of the planet in Scorpio has required that observations, to be really satisfactory, should be made within an hour or so of his meridian passage, and, apart from this, my present station is not favourable for viewing an object low in the southern sky.

I secured an observation of the spot as early as 1887, Dec. 28, when I thought it central at $20^h\ 23^m$, but the sun had risen and the markings were faint and uncertain in the mist, so that I believe the estimate was several minutes early. The last time I glimpsed the spot was on Sept. 20, when the following end was about central at $6^h\ 34^m$. The former observation was made with a 10-inch reflector, the latter with a 5¼-inch refractor. But the two best views afforded during the year were on Feb. 12, $18^h\ 40^m$, and Aug. 22, $7^h\ 5^m$, on which occasions I considered the spot precisely central, and I select these for ascertaining the rotation period. The interval separating the dates named is $191^d\ 12^h\ 25^m$, during which 463 rotations were performed, giving a mean period of $9^h\ 55^m\ 40^s{\cdot}24$. This shows that the motion of the spot has become slightly accelerated as compared with its rate during previous years. The four last oppositions have yielded the following results:—

Period of observations.	No. of rotations.	Mean Period. h	m	s
1884, Sept. 21–1885, July 8	700	9	55	39·2
1885, Oct. 24–1886, July 24......	659	9	55	41·1
1886, Nov. 23–1887, Aug. 2	609	9	55	40·5
1888, Feb. 12–1888, Aug. 22......	463	9	55	40·24

It appears that the rotation period is now nearly one second less than it was in 1885–6, though it is still six seconds greater than in 1879, when it was $9^h\ 55^m\ 34^s$. It remains to be seen whether in future years the motion of this spot will return to its former rate.

If we take the entire interval from 1886, Nov. 23 to 1888, Aug. 22 (637½ days = 1541 rotations), the mean period of the spot is $9^h\ 55^m\ 40^s{\cdot}1$, which differs little from the value $9^h\ 55^m\ 40^s{\cdot}63 = 870^\circ{\cdot}27$ daily rate, adopted by Mr. Marth in his ephemerides for the last two oppositions ('Monthly Notices,' Nov. 1886 and Nov. 1887). In August last the red spot effected its central passages about 16 minutes in advance of the zero meridian computed on the above rate.

Observations would be interesting from the southern hemisphere, where the planet is much better placed for the study of his surface phenomena.

Yours faithfully,

Bristol, 1888, Oct. 11.

W. F. Denning.

OBSERVATORIES.

Paris.—M. Loewy is at present in Algiers superintending the installation of a new equatoreal *coudé* for M. Trépied. This will be the fifth *coudé* erected since the original instrument at Paris; and as with each new trial fresh improvements are found possible, this form of instrument has already attained a high state of perfection. Certainly to see one of these instruments is to believe in them; without a personal visit it is difficult to understand how readily everything in connection with the telescope can be worked without rising from the observing-chair. Those who are beginning to find the fatigues of observing tell upon them will appreciate the fact that a long night's work can be done in this way from a comfortable seat in a warm room. M. Loewy had indeed given up observing entirely, but, thanks to the *coudé*, has found it possible to resume work regularly without the slightest discomfort. His present work is in connection with his own method of determining aberration and refraction. The two faces of a prism placed before the object-glass reflect each a star down the axis, so that both stars are visible in the field of view at once, and would remain in the same relative position so long as the prism was not disturbed, were it not for the changes in, say, refraction as the stars rise and set. The consequent change of position can, under these circumstances, be measured with a micrometer of comparatively small range, and the difference of refraction thus determined very accurately. A similar method is applicable to aberration. The pairs of stars to be observed must be selected with some care, so as to be at nearly a given distance apart and, if possible, of the same magnitude. To facilitate the selection of such pairs, which is a tedious matter by calculation, M. Loewy has resorted to the graphical method, and is constructing a large celestial globe of catalogued stars, from which he readily takes his pairs with compasses. He suggests that other observers who may wish to take part in this work, which is at the same time simple, easy, and of fundamental importance, should apply to him for lists of pairs, which, after the construction of his globe, he will be able to furnish with great ease. An interesting feature of these measures is, that stars of all magnitudes can be utilized so long as the magnitude is nearly the same for both members of a pair; so that comparatively small telescopes which happen to be furnished with a good micrometer can be utilized. There is a certain splendour observing two stars of the first magnitude in the same field.

CARLETON COLLEGE OBSERVATORY.—The 'Sidereal Messenger,' No. 68, contains a description of Carleton College Observatory, the director of which is editor of the 'Messenger.' The following brief summary will be of interest. The building stands on an eminence with abruptly sloping sides on the north, east, and south, the nearest building being the old observatory, distant 330 feet. The piers for the astronomical instruments are of stone and cement laid in solid masonry on a thick stratum of coarse gravel about 10 feet below the surface, and are entirely independent of the building. The building extends east and west 80 feet, north and south 100 feet. On the first floor is the clock-room (27 feet in diameter), in which are sidereal and mean-time clocks by Howard, a chronometer by Bond, meteorological instruments, and the telegraph apparatus which is used for the transmission of time over 12246 miles of railway. A meridian circle by Repsold, a prime vertical instrument by Fauth (3 inch), an 8¼-inch equatoreal by Clark, which it is hoped soon to use for photography, are the principal astronomical instruments. A 16-inch refractor with a Rutherfurd correcting lens is on order from Mr. Brashear. Lecture-rooms capable of seating about 70 students and a library of about 1400 bound volumes form part of the observatory. Minnesota should be proud of its "College Observatory," and we shall be pleased to hear of others following the lead of a generous unnamed Trustee of the College who has "already contributed about 2000 dollars to the support of the library and is planning larger things for the future." We congratulate our *confrère* of the 'Messenger' on the work done and wish him the future success he deserves.

PUBLICATIONS.

PLANETARY AND STELLAR STUDIES.—Mr. Gore has, by collecting various short papers of his own, produced a pleasant little book, eminently readable and containing much useful information. Had it not been for the express modesty of the title we should have liked to suggest that the book could easily have been made more complete; but the author has apparently, either from lack of time or some other reason which his readers will equally regret, not chosen to develop his "studies" into something like a connected account of his own department of astronomical work. And from another point of view it is possible that we have here a suggestion for the solution of a difficulty becoming every day more pressing, viz., the enormous increase of scientific literature and, as a natural consequence, of the number of channels through which it flows. Some method of classification, beyond mere cataloguing, will soon be found necessary, and probably the method adopted by Mr. Gore, of a worker collecting his own work, may be as useful for the busy crowd of astronomical bees, as it has already been found for some of the giants of science.

The first ten chapters of the book are devoted to the planets. Most of the known facts about each are set down carefully and briefly,—how briefly may be imagined when "Transits of Venus" are disposed of in a page and a half, although the author has seen two with his own eyes. The "stellar" studies comprise chapters on double and variable stars, nebulæ, special stars such as μ Cephei, β Leonis, and the author's Nova in Orion (which, again, is dismissed in three pages; *is* this mistaken modesty?); and on a variety of subjects from the infinity of space to the Great Pyramid. May we take exception to one of Mr. Gore's statements about space? He says "We are thus placed on the horns of a dilemma. Boundless space and a limited universe seem both beyond the mind of man." His alternatives are not exhaustive. We are not given to speculating ourselves, but we can never understand why those who enjoy that sort of amusement should find any difficulty in imagining space to be elliptic. It is only necessary to grant that the three angles of a sufficiently large triangle (say between three 20th magnitude stars) are sensibly not equal to two right angles, and space becomes at once finite and boundless.

Bulletin du Comité international permanent pour l'exécution photographique de la Carte du Ciel: deuxième fascicule.—It is to be sincerely hoped that the same energy will be shown throughout the work of construction of the photographic chart that is being exhibited by those who are discussing the preliminaries. That there must be an immense number of new problems to be solved in such a new undertaking is obvious; and it will be strange if some do not escape the vigilance of even the most forethoughtful. But we notice in the two numbers of the Bulletin already issued, and especially in the one before us, a strong determination to reduce the number and importance of such escapes to a minimum. If we may claim the privilege usually granted to spectators (perhaps as a slight acknowledgment of their trouble in looking on) of pointing out how the game should be played, we might perhaps hazard the suggestion that a little too much sympathy is being shown with the Aristotelian philosophy. We find more than once a page or two of discussion concerning questions which could readily be settled by experiment; and one cannot help feeling that, skilful as are the discutants, one would listen almost more readily to a witness who had measured and reduced one or two of the Henry plates as they are ultimately to be measured and reduced (not in a preliminary or tentative manner), and spoke from experience. On the other hand, considering the analogous nature of most astrometrical researches, we must admit that to a large extent the writers are in fact speaking from experience, and the above suggestion is not to be taken too seriously; in the case of several papers dealing directly with the result of experiment it does not apply at all.

The opening chapter of the present number, from the President, Admiral Mouchez, will be read with great satisfaction. He reports that all the instruments, including the seven being made by M. Gautier, will be ready by the spring of 1889 at latest; that the three English observatories (Greenwich, the Cape, and New Zealand) raise the total number to take part in the work to 17; that M. Vogel has satisfactorily settled the form of *réseau* to be used, and will make and verify them for £4 each; that M. Champigneulle is making good progress with the question of varnishing and preservation; and that the new American "Seed" plates will materially reduce, perhaps by one half, the time of exposure necessary. The President refers to the project of a Bureau for measuring the plates, which will be considered by the Comité Permanent at its meeting next year. Of this matter we have spoken elsewhere, or rather we have *not* spoken, confining our remarks to the previous quiet assumption that the Congress sanctioned a Catalogue of all stars to the eleventh magnitude. This assumption appears at full length in the President's introduction; and we only hope that the discussion of the misunderstanding next year will not jeopardize the real work of the Chart.

The next paper, from Dr. Vogel, which has already appeared in the 'Astronomische Nachrichten,' gives the results of experiments on forms of *réseau*, and on the deformation of the film. He finds that distortions in a distance of 1° generally amount to about 0·002 mm. or 0·003 mm., corresponding to about 0″·1 or 0″·2, but may be as much as 0·019 mm. or about 1″. These include the result of all the operations of developing, fixing, drying, &c. Fixing is found not to affect the plate seriously. The film is generally found to shrink in one direction and expand in another. To reduce the errors to insignificance he proposes to reduce the distance between the lines of the *réseau* to 5′. Then follows a suggestion by Dr. Kapteyn that the plates should be measured parallactically, so as to obtain R.A. and declination at once, instead of measuring rectangular coordinates and then converting them into R.A. and declination as proposed by Dr. Gill. The method amounts to observing the photographic plate with an equatoreal, and reading off the R.A. and declination on its circles; the plate may, in fact, be considered as replacing more conveniently the part of the heavens which it represents. Another method of the same kind would be to employ a microscope instead of a telescope. The microscope would be fixed in a kind of scale-pan, movable about the vertex in the requisite directions, and of a length equal to the focal length of the telescope. Two or more microscopes could, in fact, be fixed in the same scale-pan; and the adjustment of these at the proper distance would readily allow of the simultaneous measurement of plates which overlap and thus contain the same stars. We fancy that some trouble in adjusting plates might be saved by this method; say, for instance, that plate A was well adjusted under one microscope, another plate B might be quickly adjusted by bringing two stars common to both plates in turn under the microscopes. After A is finished another plate C could take its place, also having (other) stars in common with B, and adjusted similarly by B.

Dr. Gill replies that he is ready to accept Dr. Kapteyn's suggestions, and there follows some discussion as to the details of measurement.

Prof. Pickering's paper on Photometry comes next. He reports that a Catalogue of 1000 stars near the pole, with magnitudes deduced by eye-photometers compared with photographic measures, is nearly ready, and contains a discussion as to the effect on the latter of exposure, aperture, the use of a trace, &c. He is also examining the Pleiades in every manner possible; and again a series of 1000 stars at 2° from the equator; and will choose other groups at regular intervals.

Among minor matters, MM. Henry give their opinion that plates 3° square can be used; but Dr. Gill says that he would prefer to use only 2°, because the reduction will be made more readily in zones of 1°, and with 3° one is liable, with overlapping plates, to be concerned with 1½° at a time. We do not see, however, that there would be any difficulty in making the zones of plates overlap alternately 2° and 1°. This would indeed involve a reconsideration of the method accepted by the Congress; but we fancy that some modification of this arrangement will, in any case, be found necessary to meet the difficulty of gradually decreasing zones. The arrangement adopted by Resolution essentially assumes the same number of plates in each zone. But a very little distance from the Equator will reduce the economical number of plates for a complete zone by one, and then the trouble begins: where in the circumference is this wretched plate to be omitted?

STAR ATLAS, CONTAINING MAPS OF ALL THE STARS FROM 1 TO 6·5 MAGNITUDE BETWEEN THE NORTH POLE AND 34° SOUTH DECLINATION, AND OF ALL NEBULÆ AND STAR-CLUSTERS IN THE SAME REGION WHICH ARE VISIBLE IN TELESCOPES OF MODERATE POWERS. With explanatory text by DR. HERMANN J. KLEIN. Translated and adapted for English readers by EDMUND M'CLURE, M.A., M.R.I.A.*—The number of excellent text-books on astronomy which have of late years appeared show the extent to which this

* London, Society for the Promotion of Christian Knowledge.

most exact of all sciences is now cultivated. Not unfrequently the perusal of one of such works, taken up it may be in the course of general reading with a view to learn for the first time something of astronomy, impels a reader to endeavour to make some practical acquaintance with the subject, such as alone the reading of books cannot give. In such case, one of the first things which, after a telescope, becomes a necessity, is a good star-atlas; and although numerous atlases already exist, we have here yet another, one which also may be said to justify its appearance. In addition to the atlas, giving in 12 maps delineations of the stars from the north pole to beyond 30° of south declination, there is also a very extensive catalogue of interesting stars, double stars, variable stars, nebulæ, &c., extending to 60 pages, and including many hundred objects, which alone is valuable. In many cases considerable particulars of the various objects are given, a page concerning the great nebula in Orion, besides which the information throughout appears to be generally well up to date. For the maps the stereographic projection has been adopted for the northern portions of the heavens, and apparently a cylindrical one for the equatoreal portions; they contain all stars down to the 6½ magnitude, and there is considerable overlap in adjacent maps, in itself a great convenience; further they are not disfigured by any insertion of figures of constellations, the boundaries of which are alone lightly inserted. A large separate map of the Pleiades is given, and Bond's admirable representation of the nebula in Orion has been very well reproduced on a two-page plate; drawings of other nebulæ and of some star-clusters have also been included. The application of photography to the mapping of the stars is noticed and copies of various photographs are given, including copy of one by the Brothers Henry, of Paris. The star maps are very excellently produced, and the typography is good.

There is a somewhat brief introduction, not perhaps the best part of the work. The writer very aptly explains right ascension and declination as analogous to longitude and latitude on the Earth; but for the sake of the student it might have been added that there is also a celestial longitude and latitude reckoned in quite a different manner. And if considered necessary to include explanation of sidereal time and mean time, a troublesome thing to a student, something more complete might have been desirable. But these are minor matters, in no way detracting from the value of the other portions of the work, which we can strongly recommend as one interesting and useful to the amateur as well as worthy also to find a place in the library of the professional astronomer.

NOTES.

Mars.—M. Niesten of the Brussels Observatory, after having carefully observed the planet Mars during the opposition of 1888, is inclined to doubt M. Perrotin's conclusions as to the submerging of the continent Libya; and to attribute the apparent changes noticed at Nice to the change in aspect of the planet due to its rotation, and to the different inclination of its axis to the line of sight. Nor has M. Niesten as yet been able, with the 15-in. refractor, to divide the canals as Schiaparelli did in 1882. He contends that the white spots on the planet cannot be all snow-patches, as many of them are whitest in the Martian summer and are very near the equator. Profs. Schiaparelli and Holden aud Dr. Terby have also failed to see the supposed submersion.

The satellites of Mars were observed during the late opposition at the Lick Observatory, but under difficulties arising from the incomplete state of the dome. The following table of Mr. Keeler's gives much information as to the brightness of these objects:—

Brightness of Mars at different Times.

Date.	Remarks.	Brightness.
1877, Aug. 11	Satellites discovered by Prof. Hall at Washington.	1·91
1877, Sept. 5	Opposition.	2·31
1879, Nov. 12	Opposition.	1·22
1888, Apr. 5	Satellites first observed at Lick Observatory.	0·62
1888, Apr. 10	Opposition.	0·66
1888, June 4	Phobos seen at Lick Observatory.	0·40
1888, June 7	Both satellites seen.	0·38
1888, June 19	Deimos last seen.	0·32
1888, July 18	Phobos.	0·22
1890, May 27	Opposition.	1·15

The comparative feebleness of the system during the recent period of observation lends significance to the fact that a careful search failed to reveal any new satellites. Mr. Keeler's observations tend to confirm Prof. Pickering's conclusion that Deimos is half a magnitude brighter on the following than on the preceding side of the planet.

NEPTUNE.—In a recent number of the 'Astronomical Journal' Prof. Asaph Hall says:—

"I wish to call the attention of astronomers to the orbits of the satellite of Neptune that have been computed from the observations of different observers. Referred to the orbit of Neptune, the longitude of the node and the inclination of the orbit of the satellite are given by Mr. Marth as follows:—

Observer.	Date.	*n*.	*i*.
Lassell	1852	176°·20	148°·33
Lassell and Marth	1864	180 ·41	146 ·19
Newcomb	1874	182 ·59	144 ·04
Hall	1883	184 ·31	142 ·38

"This is a singular result, showing such large and regular variations of the node of inclination with respect to the time. There are apparently no known forces that can produce these changes, and I am inclined to think they must be caused by systematic errors in the observations. But the matter is worthy of investigation. The apparent orbit of this satellite has been very eccentric, but is now opening, and the large telescopes will be able to observe the satellite in all parts of its orbit with ease.

"As the last orbit depends on my own work, it would be better for some other observer to make a new determination.

"Neptune comes into opposition about the middle of November, and will be well situated for observation at northern observatories."

Now that so many amateurs are in possession of fine telescopes, perhaps it is not too much to hope that one of them may be able to find time for such an investigation, which might demand considerable application for a time, but does not commit the investigator to years of work.

Bibliography of Meteorology.—Mr. C. J. Sawyer, of the U.S. Signal Service, has after many years' work completed his bibliography of meteorology (which contains 50,000 independent titles) to the end of the year 1881. In his last Report Gen. Greeley, Chief Signal Officer, urged the publication of the work and expressed an opinion that it would lead to other governments co-operating by publishing a series of supplements which would secure a bibliography of the ever-increasing number of meteorological publications.

Winnecke's Comet and the Mass of Jupiter.—Dr. E. de Haertl has been engaged in investigating the observations of Winnecke's comet, with the view of determining whether they offer evidence of a resisting medium. No evidence of such medium was found by Dr. A. Möller from the observations of Faye's comet (perihelion distance 1·74); but if, as Encke thought, the density of the medium decreased as the distance from the Sun increased, comet Winnecke (perihelion distance 0·83) might still be affected by it. Dr. Haertl finds that the mean motion instead of decreasing, increases slightly, and consequently the medium has no more sensible effect on Winnecke's comet than on Faye's. It was found necessary in the course of the investigation to redetermine the mass of Jupiter; the value found was:—

$$m = 1 : 1047{\cdot}1752 \pm {\cdot}0136.$$

Meteorology at the Cape.—From the 1887 Report of the Meteorological Commission of the Cape of Good Hope we learn that the Commission are sanguine of soon issuing storm-warnings. Monthly and yearly summaries are given for 29 stations; daily reports are sent to each port, and exhibited on a map for the benefit of mariners. The Commission supply instruments, which become the property of the observer when he has made continuous observations for 5 years.

Opposition of Iris.—Dr. Gill has prepared the accompanying ephemeris of Iris and places of Comparison Stars, which he proposes to observe in conjunction with the heliometers of the northern hemisphere, and asks for observations. The conditions of the opposition this year are very favourable for a determination of the Solar Parallax.

Iris. Berlin Midnight.			Comparison Stars.							
Date.	R.A.	Dec.	No.	Mag.	R.A.	Dec.	No.	Mag.	R.A.	Dec.
	h m s	° ′			h m s	° ′			h m s	° ′
Nov. 1	2 16 1	+21 48	1	7·0	1 57 32	+17 42·5	15	8·5	2 14 18	+19 36·5
5	13 40	21 22	2	7·7	2 0 4	20 4·0	16	7·8	14 54	22 34·6
9	9 59	20 37	3	7·3	1 37	17 29·7	17	8·5	17 30	20 54·2
13	7 24	20 1	4	6·8	3 14	16 42·1	18	8·6	21 52	24 8·0
17	5 15	19 25	5	7·5	3 29	19 48·5	19	7·8	20 38	22 23·0
21	3 33	18 50	6	6·0	4 23	18 58·5	20	6·0	22 51	22 58·6
25	2 24	18 18	7	7·5	5 5	20 51·0	21	7·5	22 56	21 6·1
29	1 48	17 48	8	8·5	6 4	21 27·7	22	6·2	24 6	24 43·9
Dec. 3	1 48	17 22	9	5·5	6 32	20 41·3	23	8·0	26 3	21 48·8
7	2 2 24	+16 59	10	7·2	7 39	19 5·5	24	8·2	28 10	24 24·0
			11	7·8	8 39	22 7·0	25	8·1	28 16	22 28·3
			12	8·3	11 26	22 8·9	26	7·6	30 18	22 34·7
			13	8·0	12 17	21 21·7	27	7·0	30 32	24 9·8
			14	6·0	2 12 39	+22 39·3	28	8·3	2 31 25	+22 39·1

The magnitude of Iris at opposition is 6·7.

We are glad to hear that the Government have accepted Lord Crawford's munificent offer to present the instruments and equipment of the Dun-Echt Observatory to the nation; and the Treasury has agreed to provide means for the necessary building-operations. We gather that the new observatory, for which a site is being selected near Edinburgh, is to be regarded as replacing in some way the old observatory on the Calton Hill, which Prof. Piazzi Smyth recently despaired (owing to the hostility of Government) of being able to work properly. Proposals have been made for utilizing the latter as a training-school, or a sort of peep-show, but with what authority we do not know.

We are glad to hear that the pair of discs which Sir Howard Grubb has at last succeeded in obtaining for the 28-inch refractor for the Royal Observatory, Greenwich, are very satisfactory. He considers, indeed, that, in spite of one or two small defects, they are the finest pair he has ever seen. We may now hope that the whole telescope will soon be erected.

Mr. Common's 5-foot telescope is now at work, although not quite complete in all details. The silvering of the mirror is only temporary, but so far as observation has gone the figure is good. Many new nebulæ were picked up on the first night of observation.

Good progress is also being made with the photographic telescopes, that for the Oxford Observatory being almost ready for use. The plate-holders are to be arranged so that a field of at least three degrees square can be utilized if necessary.

Sir R. S. Ball will give a lecture on Comets and Shooting-stars on Nov. 18 at St. George's Hall, Langham Place, under the auspices of the Sunday Lecture Society.

A series of articles on the Lick Observatory appeared in 'Engineering' between July 6 and September 7.

Prof. H. Kayser of Hanover obtained a photograph of a rainbow, on a dry plate coloured with azaline, from the Righi-Kulm.

Minor Planet No. 279 was discovered by Dr. Palisa on Oct. 25.

Prof. C. A. Young is engaged on a new text-book on astronomy.

Erratum on p. 356 last line, *for* 278 read 276.

Faye's Comet, d, 1888.

Ephemeris for Berlin Midnight, by Dr. E. Lamp
(Astron. Nach. No. 2861).

	R.A.			Decl.			
1888.	h	m	s	°	′	Log *r*.	Log Δ.
Nov. 4....	7	59	53	+7	11·0	0·2754	0·1493
12....		6	55	5	38·7	·2822	·1354
20....		11	43	4	12·6	·2894	·1223
28....	8	14	13	2	55·3	0·2970	0·1101

Comet e, 1888 (*Barnard*).

Ephemeris for Berlin Midnight, by Prof. Berberich
(Astron. Nach. No. 2861).

	R.A.			Decl.			
1888.	h	m	s	°	′	Log *r*.	Log Δ.
Nov. 1....	5	23	39	+2	44·6	0·3317	0·1214
5....	5	4	28	1	30·4	·3265	·0949
9....	4	42	31	+0	16·0	·3214	·0716
13....	4	17	59	−1	5·4	·3164	·0532
17....	3	51	26	2	27·5	·3115	·0411
21....	3	23	43	3	45·7	·3067	·0366
25....	2	55	56	−4	55·3	0·3021	0·0399

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 143. DECEMBER. 1888.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, November 9, 1888.

W. H. M. Christie, M.A., F.R.S. (Astronomer Royal),
President, in the Chair.

Secretaries: E. B. Knobel and Col. Tupman.

The Minutes of the previous Meeting were read and confirmed.

Col. Tupman announced that 203 presents had been received.

Votes of thanks were passed to the donors.

Mr. Roberts then read a paper on "An Instrument for Measuring the Positions and Magnitudes of Stars on Photographs, and for Engraving them upon Metal Plates," and entered minutely into the details of the construction of the instrument and the method of using it. It was designed, he said, for the purpose of obtaining permanent copies of stellar photographs, and for printing charts of the stars upon paper with the accuracy (practically) of the original negatives, so that the charts could be utilized in astronomical researches, such as the determination of stellar magnitudes and positions. With the instrument he has demonstrated the practicability of proportionally increasing the magnitudes of the stars, so that the faintest star on a photograph is made visible to the eye, while its position is still susceptible of micrometric measurement. This is done without altering the relative positions of the stars as they are photographed; and therefore no error of distortion is introduced. He showed that by the use of twenty instruments like the one described, 23 millions of stars could be charted with great accuracy within ten years' time.

Prof. Pritchard. I have seen a few of the specimens to which Mr. Roberts refers; and perhaps it may not be undesirable that I should make a few remarks. With regard to the beautiful conception of this machine I may say that, to a certain but a very *modified extent*, this method was anticipated by me. Two years ago I exhibited at the Royal Society the stars, on a plate, pricked off by a needle-point which was attached to the micrometer, and as the micrometer moved each successive star a prick was made in the paper. Nothing in the world could be more simple or intelligible than that. The paper was examined afterwards, and we found that these pricked positions were practically as accurate as those on the original negative. But that was comparatively a mere nothing. On the other hand, Mr. Roberts has carried the thing out in a manner for which I express my intense admiration, and I look upon his device as a great step forward towards rendering stellar photography practically available for the astronomy of the present and future. It seems to me to solve the questions both of reproduction and distribution. Even the paper prints are sufficient to indicate any very sensible differences in the collocation or magnitude of stars which may have taken place, or be suspected of having taken place, whether in the near or remote future. This I hold to be an enormous stride. It possesses almost the accuracy of a photographic reproduction, without necessarily losing the faintest stars that are impressed on the original negative. Whether it be worth while to incur the vast labour and expense of reproducing such a fearfully enormous and bewildering mass of detail on Mr. Roberts's wonderful photo is a question open to discussion. At all events it is capable of very speedily reproducing in a practicable form charts of a more manageable character. In relation to what has thus been achieved by Mr. Roberts, by Mr. Common, and, I may add, by myself, by the aid of the reflector, may I express my deep regret that the Paris Conference should have dismissed reflectors in so peremptory a manner? It must be, in England at least, some considerable time before we have a practical experience of the whole capability of the photographic object-glass insisted on by the Conference. Armed with a two-foot reflector, a single observatory by this time might have made very considerable progress towards the completion and reproduction of the charts up to the eleventh magnitude, as decided on at the Paris Conference. I might add much more, but I, from prudential motives, refrain.

Rev. S. J. Perry. I should like to say a word about this instrument, as perhaps I am the only person in the room who has seen it at work. Knowing that the paper was to come on to-day, I went to Mr. Roberts's observatory; and what struck me was the same that struck Prof. Pritchard. I was in perfect admiration of the working of the instrument: a perfect idea has been carried out most perfectly by Mr. Hilger. The idea is entirely due to Mr. Roberts, and it has been beautifully carried out. The movements are perfection. It is very necessary that we should have a perfect movement for accurate measurement. The movements are as smooth as you can imagine them to be [having described the instrument on the blackboard]. The machine is so rigid that the temperature would have no perceptible effect; but if it had an effect it would be the same for the graver and the microscope, which are rigidly connected, and also for the photograph and its metal copy, which are similarly joined together. There is a very strong iron connection between the two in each case. That is a thing of immense importance. The graver has a diamond point at the end of it, and you can move it with the greatest ease and with perfect accuracy. [Blackboard illustration as to how the machine works.] There are slow motions and rapid motions for carrying the copper plate with the photographic plate; and with these slow motions the person engraving can bring at once any star into the centre of the field of the microscope. There seems to be something a little arbitrary in fixing the diameter. The image is not always circular, and therefore the engraver takes the smaller diameter of the two readings and puts the micrometer attached to the engraving obtained at that reading. The movement in this case is most beautiful, and the recording part of the instrument is wonderfully well made, as you have a perfectly steady vertical motion. I think we can say with Mr. Roberts that we are certain, within very narrow limits indeed, that every point that is visible separately under the micrometer is reproduced upon the copper plate at the same distances apart as on the original. If you look at the photographs in the hands of the Fellows you can get a good idea of the instrument, but you can scarcely realize the perfection with which it is made.

Mr. Common. I should like to say a few words on this question. This is a case where we feel the difficulty that has been frequently felt before, and that is, a Fellow reads a paper full of most interesting matter, and it is hardly possible to follow during the time he is reading it. I wish we could have some way of putting into the hands of the Fellows a rough abstract of the paper to be read. It is almost impossible to take up a thing like this, and deal with the points seriatim that the speaker brings before us. Prof. Pritchard mentions that the 11th was the magnitude that was fixed upon by the Conference. This is quite true as regards one series of plates, but the chart itself is to be to the 14th magnitude. I may say, with regard to the use of the reflector, that I quite agree with Prof. Pritchard that we might have commenced work at once with the reflector, from the fact that the reflector, with its power of bringing all the rays to one point, and the simplicity of the laws governing its aberration, is not in need of further experiments to determine its capacities; the field, it is true, is limited, but the limit of that field is always the same. It is depending on geometrical considerations only; it is independent of the thickness of the glass or any other similar consideration. At the same time that has nothing to do with the question at the present moment. The reflector will perhaps assert itself by-and-by. In the meantime, the Conference has, from other general considerations, decided to go on with the achromatic telescope with all its defects more or less. Prof. Pritchard thinks he has anticipated Mr. Roberts. I believe Mr. Roberts, eighteen months ago, exhibited at the Paris Conference some of these circles he had engraved with the instrument. He therefore had the idea then, and no doubt had had it for some time.

Prof. Pritchard. I claimed nothing.

Mr. Common. But apart from that there is the one great question: Is it the right thing to simply copy the negatives and reproduce them on paper? Is it proper to treat the whole sphere of the heavens as if we were perfectly still, and disregard right ascension and declination? Of course, if the Earth stood still, we might treat the whole heaven as an immense polyhedron, each negative being one of the facets. But, in actual fact, we are confronted with the difficulty of dealing with the right ascension and declination, which give the absolutely necessary data for identifying the stars. Mr. Roberts has brought forward an instrument which gives you a veritable copy. You have the means of reproducing the negative with what we may for the time being call absolute truth. But is that all you want? I say no. It is absolutely necessary to deal with the question of right ascension and declination.

Rev. S. J. Perry. Except that there are two motions at right angles to each other, and the plate follows the declination in one, and the right ascension in the other, with sufficient approximation to identify the stars recorded by aid of the parallel lines which are superposed on the photograph whilst the positions are being read off.

Mr. Common. I say that this is only one step towards meeting the difficulty, and there are other questions to be considered, and one of them is dealing with stars in right ascension and declination. I have not a word to say against the method, with this exception: it is extremely difficult to tell what is a star on a negative, especially when you come to the small stars. You have to go through a very long course of experience before you can tell stars from certain defects on the plate. The effect of this instrument would in some cases be to make stars out of specks of dust. If there is a speck of dust to catch the observer's eye, and he mistakes it for a star, a star will be engraved.

Mr. Common. After all, whatever you do, I think you must go back to the original negative or photographic copies of it for your information.

Mr. W. Claude Johnson. I think the only way of eliminating those defects which never fail to appear after the development of photographic films, and which are so liable to be mistaken for stars, would be to take two photographs of the same portion of the heavens. It is obvious that the chances of the coincidence of two flaws in the film in the same relative position is very remote.

Mr. Ranyard. One advantage of this method of copying stellar photographs is that the negatives are not injured. There is always the original negative to go back to. I do not sympathise with Mr.

Common in fearing that there is any practical danger of mistaking specks of dust for stars. It is quite possible that some small nebulæ might be mistaken for stars; but I think Prof. Pritchard will agree with me that there is a quality about the photographic patch representing a star as taken with a particular instrument that you cannot very well mistake. Each man gets to know what a star looks like under the microscope as taken by his instrument, and there would not be any practical danger of his being deceived by specks of dust.

The President. All members present must be agreed that this is a valuable contribution towards a solution of the question of reproducing the photographic map of the heavens which we hope to have formed in a few years' time; but I feel personally some difficulty as to which amongst the various methods would be the best for this reproduction. Mr. Roberts has proposed a method here which I think is a very good one as far as it goes, but the question of cost will enter; and there is also the difficulty of distinguishing between a speck of dust and something which may really represent a star. An alternative method, I take it, is that of photo-lithography, in which we have to sacrifice something, for no doubt we are liable to lose the faintest stars; but, on the other hand, we shall have those very faint specks represented in nebulous masses, and those who examined the photographs would not be misled by the character of the sharpness which would be seen on the copper plate. There is a good deal to be said on each side. By the method of photo-lithography we can get the actual negative reproduced perfectly. There is no possibility of error coming in; no possibility of mistakes. We may have defects in printing, or lose a certain number of stars, but what we get reproduced on the plate will be accurate in position. I must say Mr. Roberts has done a great deal towards ensuring accuracy in the ingenious device he has brought before us to-night, and I do not wish to depreciate it. But there may be mistakes, and difficulties will arise in disentangling them. I think where we want to discuss the minutiæ of the photograph that we must trust to photographic reproduction on glass, or some method like that; and if a number of glass-positives are taken, then any person who has studied any particular part of the heavens will refer to what is virtually a reproduction of the negative. I am only giving my ideas as the matter strikes me now. I think it is desirable that a method so ingenious as that of Mr. Roberts should be brought before us, and that its advantages should be considered, for undoubtedly it has many advantages.

Prof. Pritchard then read a paper on "Recent Investigations of the Stellar Parallax."

The President. You have already returned your thanks to Prof. Pritchard for this interesting paper. I feel that the work he has been doing is really most valuable. The advantage of the photographic method is that it enables the relative parallax of the star to be determined relatively to a number of stars around it, and the gist of what Prof. Pritchard has brought before us to-night seems to be that we cannot rely upon a determination of the relative parallax from any one star as giving us an absolute indication of the parallax of the brighter star; therefore I think the important point is to determine the relative parallax from a large number of stars. By taking a large number of faint stars we can obtain from the mean of the relative parallaxes so determined a fair indication of the absolute parallax of the brighter star. The determination of the absolute parallax is one beset with great difficulty, and the values which are so obtained are not to be trusted with absolute certainty. It was for that reason that I asked Prof. Pritchard the authority for the absolute determination of α Lyræ.

Prof. Pritchard. When we get our new object-glass at Oxford, I intend to set to work to find out the difference of parallax of those two stars of comparison, and that will throw very great light upon the whole question of relative parallax. It will be a difficult process, but I will make the attempt.

Mr. Sadler. I think that in the case of the proper motions of stars in the Pleiades, Dr. Elkin's observations with the heliometer at Harvard College certainly contradict those made by Prof. Pritchard (or his assistants) at Oxford.

Mr. Inwards then read a paper on a "Compensating Pendulum," which he described in great detail. It consisted of a perfectly simple pendulum formed of a rod of steel supporting a globe. The compensation was effected by rods of zinc attached to the bracket supporting the pendulum, and which caused a clip to alter its position on the lengthened supporting spring of the pendulum. The timing as well as the alteration of the proportions of the zinc and steel were done by a tangent screw, and both could be adjusted while the clock was going.

Col. Tupman asked if the pendulum had been tried.

Mr. Inwards replied that it had not.

Mr. Thwaites. Would not an alteration of the length of the pendulum affect the position of the crutch with regard to the centre of oscillation?

Mr. Inwards. It would to a certain very limited extent, and not sufficiently so to have any effect on the time, especially with a gravity escapement.

In reply to questions *Mr. Inwards* said: The pendulum spring must be grasped loosely enough to allow the clip to slide upon it, and he did not think there would be any possibility of lateral motion if the slides were well made. It might be made within $\frac{1}{1000}$th of an inch.

Mr. Common. Is not the objection to that kind of pendulum that it goes by jerks?

Mr. Inwards. I expect this would give as little trouble in this way as possible, because if that is an objection it applies to putting the tubes round the pendulum equally. The whole thing, however, fails if you cannot grasp the spring with sufficient tightness to make it firm and yet allow it to work. My own belief is that this can be done.

The President. It is a very important matter in connection with astronomy to have the compensation of the pendulum improved and brought to perfection. I do not altogether agree with Mr. Common's remarks as to the present compensating pendulum, which is composed of steel and zinc, going by jerks, because the experience we have had at Greenwich—I believe it is the experience of other astronomers—is that its action is very uniform and satisfactory. I have no doubt in the old gridiron pendulum there was some difficulty of that sort, but that may have arisen from the peculiar form of construction. With the present zinc and steel pendulum the compensation is very good indeed, but there is the difficulty Mr. Inwards has pointed out of adjusting the compensation perfectly. If his method would answer, and if the clips which hold the spring could be made to move with sufficient delicacy in the slide so that they would not partake of the oscillation of the pendulum, it would be a very satisfactory solution of the question of a compensating pendulum. I hope Mr. Inwards will be able to put his ideas into practical working order.

Mr. Knobel. I think it will be a satisfaction to the Fellows to learn that the third volume of Dr. Auwers' re-reduction of Bradley's observations, containing the catalogue of stars, has at last been published. The volume has not yet been received by the Society, but I have received an advance copy, and I think the Fellows will be interested to see this important result of Dr. Auwers' labours.

Mr. Espin then read a paper on the spectrum of Mira Ceti.

Mr. Maunder. I observed the spectrum of Mira Ceti on Oct. 5, and saw a bright line in the violet, and obtained ten measures of its place. I did not recognize the line at the time of observation, but on reducing the measures the next morning, found that it was the violet line of hydrogen, close by G. I was not able to detect the fourth line of hydrogen, the one further in the violet, as the star was scarcely bright enough to enable me to trace its spectrum so far. I looked carefully for F, D_3, and C, but could not see any bright lines at or near their places. The spectrum of the star as a whole was very strongly marked; the zones, or bright spaces between the bands, being very bright and free from absorption lines, and the dark bands themselves standing out in great distinctness on the background of the continuous spectrum.

On Oct. 1 I also saw the F line bright in the spectrum of R Cygni, though it must, I should judge, have faded very much from the brilliancy it had presented when Mr. Espin discovered it earlier in the year.

The following papers were announced:—

J. Tebbutt. "Results of Micrometric Comparisons of Jupiter and β Scorpii in May 1888."

W. F. Denning. "Height of a Perseid Fireball."

Rev. A. Freeman. "Note on the Occultation of χ Orionis, Oct. 24, 1888."

I. Roberts. "On an Instrument for measuring the positions and magnitudes of Stars on Photographs, and for engraving them upon Metal Plates."

Prof. C. Pritchard. "Results of recent investigations of Stellar Parallax, made at the University Observatory, Oxford."

R. Inwards. "On a Compensating Pendulum."

Rev. T. E. Espin. "On the Spectra of R Cygni and Mira Ceti, and some Stars with probably similar Spectra."

A. Marth. "Ephemerides of the Satellites of Saturn, 1888–89 (concluded)."

A. Marth. "Ephemeris for Physical Observations of the Moon, 1889, Jan. 1 to April 1."

Radcliffe Observatory, Oxford. "Observations of Comet *a* 1888 (Sawerthal)."

Lieut.-Gen. Tennant. "A Table of the positions of Observatories, with Constants useful in correcting Extra-Meridian Observations for Parallax."

Rev. W. J. Crofton. "Observations of Comet *e* 1888 (Barnard), made at Stonyhurst College Observatory."

Rev. S. J. Perry. "Observations of Occultations of Stars by the Moon, taken at Stonyhurst."

Rev. S. J. Perry. "Observations of Jupiter's Satellites made at the Stonyhurst Observatory."

F. Bolton, Calcutta; *Andrew Claude de la Cherois Crommelin, B.A.*, Trinity College, Cambridge; and *Arthur Herbert Leahy, M.A.*, Pembroke College, Cambridge, were duly elected Fellows of the Society.

The following gentlemen were proposed for election as Fellows of the Society:—

Henry Lord Boulton, Carácas, Venezuela. (Proposed by Sir H. Grubb.)

Ernest William Brown, B.A., Christ's College, Cambridge. (Proposed by G. H. Darwin.)

Samuel Fellows, Tynwald Villas, Lower Villier Street, Wolverhampton. (Proposed by Joseph Hough.)

John James Dewis Goodridge, Bevis Mount, Southampton. (Proposed by John Newton.)

Frederick William Nash, Holmesdale, Birchfield Road, Birmingham. (Proposed by James Leigh.)

Jesse S. Nimkey, The Cottage, Sewardstone, Waltham Abbey, Essex. (Proposed by Herbert Sadler.)

William Schooling, Brechin House, Rivercourt, Hammersmith, W. (Proposed by James S. Cooke.)

MEETING OF THE LIVERPOOL ASTRONOMICAL SOCIETY.

W. H. Davies, Esq., F.R.A.S., in the Chair.

The second meeting of the eighth session was held at Colquitt Street on Monday evening, November 12, 1888. The minutes of the last meeting were read and confirmed. Several candidates were proposed for membership, and those proposed at the last meeting were elected.

The Secretary read a most interesting paper by Mr. W. H. Monck, F.R.A.S., on the Limits of the Stellar Universe. The discussion which followed was warm, and on the whole antagonistic to Mr. Monck's views.

A paper by Mr. K. J. Tarrant on "Double-Star Observations" was also read; and the Chairman read a letter from Mr. Tarrant, in which he asked amateurs to give greater attention to double-star work, and suggested many methods by which good work might be done by even those possessing but small instruments.

Mr. W. F. Denning's report on the work of the Jupiter Section was most interesting. From the report Mr. Williams is to be congratulated on the really capital work he is doing.

After the usual vote of thanks the meeting terminated.

MEETING OF THE ASTRONOMICAL SOCIETY OF FRANCE.

1888, November 3.

M. C. Flammarion, *President*, in the Chair.

M. Ricco sent a communication with reference to observations of the great sun-spot of 1882, Nov. 12–25, which he stated was the largest ever observed, its area being more than 50 times that of a great circle of the Earth. In some parts it exhibited rose-coloured veils and the nuclei were crossed by large yellow arcs.

M. Gaudibert sent a drawing of Eratosthenes, in which 12 hills were shown within the ring, and a great number of small craters on the east.

M. Blot communicated a demonstration of the formula

$$v^2=\mu\left(\frac{2}{R}-\frac{1}{a}\right),$$

which gives the velocity of a planet in terms of the major axis and of the radius vector.

M. Guiot, of Soissons, saw Vesta with the naked eye from Sept. 5–20, and observed the companion of Sirius with a $3\frac{3}{4}$-in. refractor.

Among other communications were observations of meteors from

Majorca, the Gulf of Juan and Troyes; observations of sun-spots. Comet α 1888, the lunar eclipse of July 23, and occultation of a 7th-magnitude star by Jupiter.

The Flammarion Astronomical Societies of Marseilles, Argentan, Brussels, Jaën, and Bogota were elected corresponding Societies.

MEETING OF THE ROYAL METEOROLOGICAL SOCIETY.

THE first Monthly Meeting of this Society for the present Session was held on Wednesday evening, the 21st instant, at the Institution of Civil Engineers, 25 Great George Street, Westminster; Dr. W. Marcet, F.R.S., President, in the Chair.

Señor A. Arcimis, Mr. J. W. H. Gray, Dr. J. L. Green, Mr. R. T. Morgan, Mr. C. E. Mumford, Mr. E. L. Oxenham, F.R.G.S., Dr. A. M. Robertson, Dr. E. Seaton, Mr. J. N. Sidebotham, and Dr. T. C. Squance were elected Fellows of the Society.

The following papers were read:—

1. "Results of an Investigation of the Phenomena of English Thunderstorms during the years 1857–59," by Mr. G. J. Symons, F.R.S. This paper was written nearly thirty years ago; it has now been communicated to the Society at the request of the Thunderstorm Committee. The paper contains a summary, chiefly in statistical form, of some of the results of an investigation into English thunderstorms and the accidents produced by lightning during the years 1857–59. The author found that in sheet-lightning the most prevalent colour is white; then yellow, blue, and red. In forked lightning the order is nearly reversed, blue being more than twice as frequent as any other colour; then red, white, and most rarely yellow. Sheet-lightning was seen about twice as often as forked.

2. "Notes on the Meeting of the International Meteorological Committee at Zurich in September 1888," by Mr. R. H. Scott, F.R.S. The Committee recommended certain rules for the publication of data by travellers &c., so as to ensure their being useful for the advancement of sound climatological knowledge. The proposals for an international cloud-nomenclature, as recommended by Mr. Abercromby and Prof. Hildebrandsson, did not commend themselves to the Committee, who suggested that the subject should be further studied. At the conclusion of the Meeting the Committee was dissolved.

3. "On a Method of Photographing Cirrus Clouds," by Dr. A. Riggenbach. The author exhibited some photographs of cirrus and other fine clouds which had been obtained by using the surface of a lake as a polarizing mirror.

Mr. A. C. Stratten exhibited some models of very large hailstones—spheres about 2½ inches in diameter—which fell at Montereau, about 40 miles south-east of Paris, on August 15th, 1888.

Meteor Notes.

THE Geminids are due in Dec. (max. 9–12), and the progress of this display should be watched during the first half of the month. Until the 9th, observations should be made before midnight, but after that date moonlight interferes and the shower can be well seen in the morning hours. There appear to be several active radiants in Gemini at this epoch (see 'Observatory,' April 1886, p. 155), but the principal centre is at 107° +33°. There are also some showers of Taurids in the early part of December, which require more observation; one near ζ Tauri was especially rich in 1876 and few following years. After the middle of December, when the Geminids have ceased, meteors generally become scarce, and this relative scarcity appears to be sustained until July, when the horary numbers always exhibit a great increase.

Recent Observations.—At Bristol on November 13 the eastern sky was watched between $15^h\ 30^m$ and 18^h for Leonids. 17 were recorded in the 2½ hours, and there were 12 other meteors which belonged to the minor streams of this important epoch. The Leonids were of the usual type, viz. bright, swift meteors with very pronounced and durable streaks. The point of radiation was very satisfactorily defined at 149° +22°. Several of the meteors were unusually large; one observed at $16^h\ 55^m$, which appeared near the S. horizon, caused a glare over the whole sky, and must have been an excessively brilliant object in the English Channel and the N.W. parts of France. Mr. Backhouse at Sunderland also re-observed the Leonids on November 13, between $16^h\ 48^m$ and $18^h\ 14^m$, and found them "unusually numerous." The flash of a very brilliant one at $17^h\ 19^m$ was noticed, and its streak remained visible to the eye for 9 minutes. A Leonid = ♃ was seen at Bristol on the same night at $17^h\ 19^m$, and there is no doubt the two observations relate to the same body. The streak as observed at Bristol only endured a few seconds; but this may be accounted for by the fact that the meteor was at a very great distance (more than 350 miles) from the Bristol observer and that its position was extremely close to the horizon. At Sunderland the meteor was more elevated, being much nearer to the observer, and its brightest display occurred near Arcturus. The durable part of the streak was projected at a point just preceding the stars π and ζ Boötis. Several other fine Leonids were remarked by Mr. Backhouse, and it is certain the late recurrence of this shower has been more decided than any other since 1879, when it was presented with similar strength. We are now gradually drawing nearer to the maximum in 1899, and it will be useful to secure annual observations with the purpose of finding whether there is a progressive increase in the numerical strength of the stream.

Large Meteor.

A fine meteor, > ♀, was seen by Mr. Booth at Leeds on October 30; its path was from 159° +58° to 181° +51°, which it traversed rather slowly. A large, though different, meteor was also seen at Bristol on Oct. 30 at 9^h 56^m. It was = ♃, and fell very slowly from 82° + 15° to 94° + 9½°. It is in a high degree probable that this pair of bolides belonged to the same stream of Taurids (radiant about 58° + 23°) which has supplied us with many fireballs at about this epoch; but no duplicate accounts of either of the bright meteors referred to have been received, so that their real paths and radiants cannot be definitely computed.

Bristol, 1888, Nov. 19. W. F. Denning.

Selenographical Notes.

Photograph of the Moon taken with the Great Lick Telescope.—Through the kindness of Prof. Holden, I have received a photograph of the Moon, about 5½ in. in diameter, taken with this instrument on August 13, which, as regards sharpness and general distinctness, is by far the most perfect example that has come under my notice. The long. of the morning terminator at the time was apparently from 7° to 8° west, and at this phase of illumination many of the light rays emanating from the neighbourhood of Tycho can be plainly traced, notably that which traverses the eastern side of Fracastorius and strikes across the Mare Nectaris. A very cursory examination of this beautiful photograph reveals many features for which we may search in vain in others. In Fracastorius, for example, the small crater near the north end of the west wall is very apparent; the doubtful object near the centre of the floor is represented by four minute white spots (probably mountain-peaks); the crater *f*, north of this, may be described as prominent; and the seemingly breached rings at the north end of N.E. wall are easily traced. As regards rills and light markings, to those who are familiar with its position, the curved rill on the S.E. quarter of the interior can be distinctly made out, as can also the cleft-like valley which cuts through the south border and then trends towards the south-east. The dark streak crossing the floor from west to east, and the curious hook-shaped marking, both north of the centre, are also clearly shown. In the neighbouring formation Beaumont, several of the larger craterlets on the floor are traceable without difficulty. The details of the great walled-plains Theophilus, Cyrillus, and Catherina are exquisitely portrayed; and in the adjoining formation, Mädler, the ridge (spoken of in 'Observatory,' No. 119, pp. 67, 68) running up to the gap in the north wall is clearly followed through the opening and over the eastern side of the floor. The most striking and pleasing peculiarity of this photograph is that every object appears to be "in focus," and admirably distinct, except those very near the terminator or close to the limb. It is also unusually free from flaws or blemishes. Students of the Moon's surface may, it is hoped, look forward to a complete series of these wonderful pictures, which will be of inestimable use in supplying them with accurate outlines and positions of the grosser features. I may add that the western section of the great Ariadæus rill is shown as a dark, coarse, ill-defined line, and the mountains abutting on it are clearly distinguishable. Other narrower clefts in the neighbourhood do not, however, appear to have impressed themselves on the plate.

Fontinelle.—On November 13th, 8^h 10^m to 8^h 40^m, I noted a bright little crater near the crest of the ring of this formation on the north-west side. It does not appear in any map, and has probably been hitherto overlooked. Two other small craters were seen at the foot of the slope of the north wall. Schmidt shows one of these objects only. The discrepancy between Schmidt's map of this region and those of Mädler and Neison is very marked and requires investigation. The latter observers appear to exaggerate the extent of the parallelism which obtains to a certain degree among the ridges and mounds in this neighbourhood. Mädler's square formation on the west is, in truth, far less regular than he represents it, and more in accordance with Schmidt's drawing. About one diameter due west of Fontinelle there is a dark spot surrounded for some distance by a dusky surface. It appears to be exactly on the site of the cross-marking ζ (Neison, Map vi.), and is visible under a comparatively high sun. I have sometimes fancied that it is encircled by an irregular border, and that it is a deep depression, but the dinginess of its immediate surroundings makes it difficult to decide as to its true character.

Hercules.—Mr. W. H. Maw, F.R.A.S., observing this formation some months ago with his 6-in. Cooke-refractor, noted that the crater e on the south-east wall is clearly double, consisting of two craters side by side. In more recent views, however, and under lower illumination, it has sometimes appeared single and much elongated in a direction radial towards D, the large crater on the floor. On November 11th the double craters were again noted. They were very bright and uniform in size. Mr. Maw considers that the more westerly of the two is that which is generally visible, and that its companion is only seen during a short time.

Thos. Gwyn Elger.

Kempston, Beds, 1888, November 20.

Southern Star Spectra.

In the present dearth of information about the spectra of southern stars, the results of some desultory observations upon them which, by the kindness of Dr. Gill, I have been enabled to make during my stay here, may possess an interest they could not otherwise claim. The instrument placed at my disposal for the purpose is the 7-inch Merz equatoreal of the Royal Observatory, with a direct-vision prism placed behind the eyepiece. Neither slit nor cylindrical lens has been employed, and the necessary breadth has been given to the spectrum by putting it slightly out of focus. No estimates, far less measurements, of wave-lengths have been attempted.

The first sight of the spectrum of γ Argûs is something to be remembered. The three bright rays—two golden, the third of a deep cobalt-blue—disclosed to M. Respighi and Mr. Lockyer at Madras in 1871, literally blaze out from a vivid background of uniformly dispersed light. A fourth line in the orange (of wave-length 590, according to Dr. Copeland) is less conspicuous. There is a considerable amount of absorption beginning in the green, and extending up to, if not beyond, the blue line superposed upon it. Repeated examinations have convinced me that the failure of light apparent in this region is not illusory. Further up, the spectrum makes, as it were, a fresh start, and a long sweep of intense violet light is obviously terminated only by visual insensibility to its shortening vibrations. A photograph of the spectrum of this star might be decisive on many points of its physical constitution.

There appears to be no doubt as to the reality of the gain in lustre of η Argûs lately noticed by Mr. Tebbutt. Its magnitude was determined here by Mr. Finlay, in March 1886, at 7·6; it is now fully 7, and the object stands out at a glance from the numerous surrounding stars both by the superior intensity and by the reddish glow of its light. Indications of dark shadings are perceptible in its spectrum, which at present includes no bright lines. More powerful optical means than those used by me would be requisite for the satisfactory establishment of its spectroscopic character, which may very likely prove to be subject to striking changes. The desirability of obtaining a reliable record of its actual phase is hence evident.

In the course of my explorations, I have encountered some fine specimens of banded spectra, not (to the best of my knowledge) previously observed. I subjoin a few notes regarding them.

L_2 Puppis was discovered by Dr. Gould in 1872 to vary from 3·6 to 6·3 magnitude in a period of 136 days, the change being "rapid at the maximum, and comparatively slow near the minimum, which apparently occurs about six days nearer to the preceding than the following maximum. The star," he adds, "is red in all its stages, and remarkably so when faint" *.

My observation of it was made in the morning of October 22, just at the date of the *computed* minimum ensuing upon the maximum of August 20, as given in the 'Companion to the Observatory.' The star seemed, however, unduly bright; its magnitude was certainly above the fifth. I am not aware that a maximum has been *observed* since that of March 16, 1878; nor does the adopted formula of change appear to be at present even approximately conformed to. Further observations of this interesting object are much needed. Its spectrum shows a magnificent columnar range of bands, sharply terminated towards the violet, and of extraordinary intensity. They seemed a precise repetition of the rhythmical shadow-spaces in the analyzed light of Mira Ceti which I had observed about an hour previously. The famous variable was then visibly on the decline. Its magnitude might be put roughly at 4·5, while on October 1, three days after the maximum due on September 28, it was judged somewhat above the fourth. No traces of bright lines were seen on either occasion. L_2 Puppis is quite as conspicuously red a star as Mira. The physical resemblance between the two objects is especially noteworthy considering their very different laws of light-change.

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* Uranometria Argentina, p. 279.

The spectrum of β Gruis, a reddish star of the second magnitude, is of the third type. It includes seven or eight strong bands, exquisitely gradated towards the red. There is strong absorption in the blue, but a tolerably strong gleam of violet light shows beyond.

The spectrum of δ^2 Gruis is similar. It is a very red star of 4·2 magnitude, with a ninth magnitude bluish attendant at 61″, giving an almost evanescent spectrum, which seemed to me, on October 7, when I first examined it, to include *only* the more refrangible rays. If this be the case, its colour is real, and not merely subjective.

The colour of π^1 Gruis was noted by Dr. Gould as "deep crimson," that of its neighbour π^2 as "conspicuously white." The two objects contrast most strikingly when brought into the same field of view. The spectrum of π^1 is deeply banded, and belongs almost certainly to the scantily represented fourth type. The star shows no sign of variability. Its magnitude was fixed by Gould at 6·7.

The spectrum of ν Tucanæ is delicately shaded with bands on the pattern of those of α Scorpii. It is a reddish star shining with approximate constancy as of the fourth magnitude.

H Scorpii, too, shows a spectrum modelled on that of α in the same constellation. Fluctuations from 4·4 to 5 magnitude, imputed to it by Gould, have not so far been recognized by its insertion in any catalogue of variables.

The "beautifully banded" spectrum of π Puppis, observed by Dr. Copeland in 1883, would appear to have lost much of its distinctive character. Traces of bands can now with difficulty be made out; and it is worth notice that M. Pechüle, in 1882, referred the star to the solar class. Its light has a perceptible coppery tinge, and varies, by Gould's estimation, from 2·4 to 2·9 magnitude.

R Doradûs was found, by the same observer, to fluctuate, in an undetermined period, from 5½ to 6¾ magnitude. This extraordinary object strikes the eye with the glare of a stormy sunset, and its spectrum is correspondingly deficient in rays of high refrangibility. It shows marked interruptions, especially in the green; but the effect of a range of vivid lines may be due to the presence of deep groovings by absorption.

The colour of R Sculptoris justifies Gould's qualification of it as "an intense scarlet." The star glows like a live coal in the field. I looked at it for the first time this morning, when it *ought* to have been at a minimum. It was, nevertheless, still fully of the sixth magnitude. The period assigned by Gould to its change from 5·8 to 7·7 magnitude is 207 days, and the last maximum was timed to occur on July 4 (see 'Companion to the Observatory' for 1888). The spectrum, as was to be expected, is a remarkable one. It includes virtually *no* blue light, and the drifting clouds which interrupted my view only partially veiled several wide dark intervals in the red and green. I hope before leaving the Cape to have an opportunity of re-examining this star as well as R Doradûs.

Royal Observatory,
Cape of Good Hope,
October 24, 1888.

A. M. Clerke.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Cometary Statistics.

Gentlemen,—

In lately examining catalogues of comets with a view of deducing some conclusions from them, I noticed a fact which may either indicate a physical peculiarity of comets or an accident of observation; which for that reason I desire to submit to the consideration of your readers. It is that the perihelia of the great majority of comets whose orbits have been computed lie to the north of the ecliptic. It is difficult to give exact data on this point, owing to questions as to the identity of certain comets and doubts as to the orbits of others; but according to my reckoning up to the end of the year 1884 there were 177 comets with northern perihelia against 115 with southern.

Now it occurred to me that the explanation of this fact might be the following:—Our observing stations are chiefly situated in northern latitudes. Comets are rarely visible when very remote from their perihelia; therefore comets which pass their perihelia north of the ecliptic are more likely to be detected and observed than comets which pass their perihelia to the south of it. It is some confirmation of this that up to the year 1700, when comets were rarely observed unless visible to the naked eye, the numbers were 37 to 18, giving a proportion of more than two to one. It is somewhat singular, however, that after having been equalized between the years 1770 and 1820 the comets with northern perihelia have again asserted their superiority since the latter date. Moreover, on examining the 18 comets previous to A.D. 1700 whose perihelia were south of the ecliptic I find that 10 passed the perihelion during the winter half-year, contrary to what we would expect if the result was due to an accident of observation.

As the point towards which the Sun is moving lies to the north of the ecliptic, it might be expected that more comets would on the whole come to us from the north than from the south. But a comet coming from the north will usually have its aphelion north and its perihelion south. The fact that three fifths of the comets have their perihelia to the north and their aphelia to the south thus becomes more significant, and I can hardly regard it as *wholly* the result of the position of our observing stations.

These remarks render it necessary to use cometary statistics with caution, unless northern are distinguished from southern comets in them. The large preponderance of one class might lead us to attribute to comets generally some property which is only true of that class, and the preponderance of the class itself may be due to accidents of observation rather than physical causes.

16 Earlsfort Terrace, W. H. S. Monck.
Dublin, Nov. 10.

Jumping Stars.

Gentlemen,—

Your note on jumping stars has recalled to me phenomena which I met with many years ago on the Indian Survey. I had intended seeking farther details as to the phenomena described; but various matters have caused it to escape me, till it is now too late to do this for the next number of your periodical.

That objects the rays from which pass near the surface of the earth were very difficult to observe, and that they seemed to move horizontally with reference to the wire with which intersections were made, was a common experience; in fact the uncertainty of measuring angles in the plain country was mainly due to these phenomena. When the definition was bearable, the motions small, and their period not very long, it was possible to estimate a mean place with moderate accuracy. Usually uncertainty of direction was accompanied by diffusion, but it was not always so. I have seen fairly defined objects wander about so that after spending a minute or two in estimating a mean place, and reading, a re-examination showed that the position had entirely changed. Water lying between the observer and object, or a river, always produced these phenomena, so did a sandy soil, especially bare sand (probably from underlying water), and rapid change of temperature was certainly a predisposing cause.

The most remarkable instance I recollect was when working on the north side of the Runn of Cutch. I was, with the late Colonel Strange, in the observatory soon after sunrise, and our attention was directed to objects, among others a heliostat, in the south. The heliostat was well defined and to all appearance easily intersected, but a short watching showed that it was in motion; we saw it traverse the field to 20′ or 30′ from its first observed place with an irregular motion, then return and pass the other way. Clearly observation of any sort was hopeless and had to be discontinued.

Of course there was vertical oscillation, but of that no notice was taken; one always had to deal with objects in vertical motion.

With reference to Herr Weyer's phenomenon, I have sometimes fancied for a few seconds that I saw abnormal motions of a bright object, even at some height, but, of course, it vanished on any test being applied. It is more difficult to explain the Australian phenomenon, where several people saw the same thing; far more information as to the circumstances is desirable. How did the belief arise that the Sun dances on Easter morning? Had it any similar foundation in the impression of lookers-on? One can readily understand that once such a phenomenon was asserted to occur, witnesses would not be wanting. Yours faithfully,

Ealing, 1888, Oct. J. F. TENNANT.

GENTLEMEN,—

Many years ago I witnessed the phenomenon referred to by your correspondents in Nos. 141 and 142. On an autumnal evening, after twilight had ceased, I observed Jupiter about 15° above the horizon, towards the east, oscillating to the extent of half a degree in all directions. I did not observe the same movement in any of the stars, none of any magnitude being visible in that part of the heavens. I have no doubt Mr. W. H. Maw's theory is the true explanation. In this case, after a warm day, the heated air rising from a town intervening between myself and the object, and mixing with a cold stratum above, was probably the cause of the strange phenomenon. Yours faithfully,

Dover, Nov. 11th, 1888. WILLIAM COLEMAN.

[MR. ARTHUR SEARLE contributes a note on this subject to the Astr. Nach. No. 2863. He noticed the phenomenon in the case of Capella; but though another observer recognized the instability at once, he did not generally agree with Mr. Searle as to the direction of motion. Mr. Searle is therefore inclined to regard the phenomenon as subjective, and to attribute it merely to the want of distinct points of reference near the star upon which the eye could be fixed. It is, however, quite possible that the difference noticed between eye-observations and telescopic, or between eye-observations a few feet apart, is due to the smallness of the pencil of light received by the eye. If the irregularities of temperature are very local, it may be that two small parallel pencils at a few feet, or even a few inches, distant may be affected quite differently. By using a telescope we get a mean or integrated effect. On this hypothesis we should expect the steadiness of the centre of the image to increase with the aperture; and to all appearance this is often the case. I am not now speaking of definition, but of the excursions of a star bodily to and fro—such as are familiar to observers of polar stars, for instance, and which appear larger (in seconds of arc) with small instruments.—H. H. T.]

Opposition of Iris.

Gentlemen,—

I have been engaged since the beginning of September in observing the stars selected by Dr. Gill as comparison-stars for his observations of Iris. The list he sent us is the same as that published in the last number of the 'Observatory,' except that your No. 18 does not appear in our list, and the latter has two additional stars which I may call 12*a* and 17*a*. The places of these are, for 1888:—

	Mag.	R.A. h m s	Decl. ° ′
12*a*....	6·0	2 11 55	+19 33·0
17*a*....	7·3	2 19 27	+24 39·0

Although the stars follow each other so rapidly that it is possible to observe only five or six of them each night, still I had hoped to have observed all the stars this season twice in each position of the meridian-circle. The weather, however, has been so bad at this observatory that it is very likely I shall not be able to complete the work.

In case the weather remains as unpropitious as it has been of late, it may be well to state how my observations stand at present; so that if anybody else is engaged in the same work he may direct his efforts *first* to those stars which have hitherto most eluded me. The following is an abstract of my observations (D.=Dunsink, Gr.=Greenwich):—

Star.	No. of obs. D.	No. of obs. Gr.	Star.	No. of obs. D.	No. of obs. Gr.	Star.	No. of obs. D.	No. of obs. Gr.	Star.	No. of obs. D.	No. of obs. Gr.
1 ..	4	7	9 ..	4	0	15 ..	2	0	22 ..	2	1
2 ..	4	5	10 ..	2	0	16 ..	0	0	23 ..	3	2
3 ..	4	7	11 ..	2	3	17 ..	4	2	24 ..	2	0
4 ..	4	1	12 ..	4	2	18 ..	4	0	25 ..	1	1
5 ..	2	0	12*a*.	1	1	19 ..	2	0	26 ..	2	0
6 ..	1	7	13 ..	2	0	20 ..	4	2	27 ..	2	0
7 ..	1	7	14 ..	3	4	21 ..	1	0	28 ..	1	3
8 ..	0	1									

There is a star of the 7·5 magnitude 2^{s}·8 *p.* No. 27, and a few seconds north of it. This is D.M.+24°·375. It might easily be observed by mistake for No. 27.

The declination of No. 16 is +22° 54′·6, not 22° 34′·6 as given by Dr. Gill.

Yours faithfully,

Dunsink, Dublin, 1888, Nov. 23. Arthur A. Rambaut.

[We have added the number of observations at Greenwich to the table given by Mr. Rambaut. With respect to the two stars 12*a* and 17*a*, the former was omitted from the list we first received, and No. 18 was substituted for Mr. Rambaut's 17*a* by Dr. Gill's *desire.—Eds.*]

Comet Nomenclature.

Gentlemen,—

I have long been at loss to understand by what principle astronomers are guided in naming comets after their discoverers. At one time those comets only which were invisible to the naked eye on the date of discovery were distinguished by the names of their discoverers, but now no rule seems to obtain, but that of the veriest caprice. My attention has only recently been again drawn to this subject by a communication in No. 140 of the 'Observatory.' That communication contains a list of eight comets, which all became visible to the unassisted eye. Of these comets, nos. 1 4, and 8 were discovered without a telescope. Nos. 1 and 8 were at first only suspected to be comets, and required to have their cometary character established by optical aid. Now it is noteworthy that in this list the writer has, except in the case of the Great Comet *b* of 1881, retained the name of the discoverer where such name was certainly known. What, may I reasonably ask, is the cause of this now stereotyped omission? The omission appears strange in this instance, when we consider that the mere discoveries of the Comets *b* 1881 and *a* 1888 were made under precisely similar circumstances (see the 'Observatory,' No. 52, p. 239, and the R.A.S. 'Notices' for March last, p. 295). Stranger still does the omission appear when it is considered that in the case of the Comet of 1888 the discoverer merely announced his discovery to others, and left them to fix the comet's position; while in the case of the Great Comet of 1881, the first accurate position was secured by the discoverer and immediately telegraphed to other observatories. He also obtained a good set of accurate places, and calculated an approximate orbit from his own observations. Under these circumstances it would not be too much to repeat my inquiry. Let it be distinctly understood that I do not complain of a casual omission, but of the persistent manner in which the Great Comets of 1861 and 1881 are dissociated from the name of their discoverer. The custom of naming comets after their discoverers should either be impartially adhered to or altogether abolished.

With reference to your remarks on Encke's Comet in No. 140 of the 'Observatory,' p. 354, I may say that the Director of the Melbourne Observatory asked me if I thought it would be necessary to telegraph my observation to Europe. Considering that the discovery was only that of a well-known and certainly expected comet, I did not think that the expense of a cable-message to Europe should be incurred. Under these circumstances no cable-message was forwarded, but a written memorandum of the discovery was dispatched by the first mail both to your own Journal and to the 'Astronomische Nachrichten.' Yours faithfully,

Windsor, N. S. Wales,
1888, Oct. 16.

John Tebbutt.

[Erratum in the 'Observatory,' No. 138, p. 300, line 6:—
For $8^{s}\cdot 30$ east *read* $8^{s}\cdot 30$ west.

First Discovery of the Great Comet of 1680.

GENTLEMEN,—

There is a circumstance with regard to the first discovery of this remarkable comet which I have not seen alluded to in any English book, or, indeed, anywhere else except in Kirch's own pamphlet 'Neue Himmels Zeitung,' published at Nuremberg in 1681, and giving an account of his discovery and observations. An abstract of this in Latin (presumably by Halley, then Secretary of the Royal Society) is given in the 'Philosophical Transactions' for 1714 (vol. xxix. p. 170), but no mention is there made of the circumstance to which I allude.

Kirch noticed the comet first at Coburg, early on the morning of the 14th (corresponding to the 4th O.S.) of November, 1680, and seems to have felt a natural pride at being the first to detect a comet with the assistance of a telescope before it had been seen with the naked eye. It was, at the time, not far from the planet Mars, and was just visible to the naked eye. At first he doubted whether it was a new comet, or a nebula similar to that in the girdle of Andromeda; but its motion soon decided that it was the former. "Accidit autem," says Halley, "nescio quo fato, ut Cometa hic (quem vespertinum tantopere prosecuti sunt Cœlispices) antequam Solem attigerat matutinus, nec *Parisiis* neque *Grenovici* ne semel quidem observatus sit; quique eum viderunt et observarunt, incongrua et inter se pugnantia, ac pro rei subtilitate parum idonea prodidere; neque ante *Novemb.* 17, mane a quopiam Observatorum visus est." A few days after Kirch's discovery, he tells us he noticed in the Nuremberg journals an announcement from London, dated October 25, that a comet had been seen on the previous Saturday. At first this caused him no surprise, because he thought that the comet, though faint, might have been seen at one of the English observatories. But as nothing further appeared on the subject, he set the report down as "ein blosses Geschrei."

Now the date of the London announcement is doubtless in Old Style, by which October 25 would correspond to November 4, the day on which Kirch actually found the comet. The previous Saturday would be Oct. 21, O.S., or Oct. 31, N.S. Although it is of course quite possible that some one in England had seen a meteor on that day and spoken of it as a comet, it would surely have been a very remarkable coincidence that a mere "Geschrei" of the appearance of a comet should have been started in London on the day on which one was actually seen (just visible to the naked eye) in Germany. Halley, indeed, appears to allude to some scarcely intelligible reports; but it is possible that some one with very acute sight may have seen the comet. Does any record remain of the actual words of the English announcement in the Nuremberg journals?

Evelyn in his Diary mentions having noticed, on the evening of the 12th (22nd N.S.) of December 1680, "a meteor of an obscure bright colour, very much in shape like the blade of a sword." There is no doubt that he means this comet, though it is not so easy to be sure to what he refers when he compares it to one seen by himself in 1640. It is true that the Chinese annals mention a comet seen in December of that year, but it does not seem to have been conspicuous. When he adds, in December 1680, that "we have had of late several comets," he is probably referring principally to the comet discovered by Kirch and seen by others also in the previous month, not being aware that it was the same body before perihelion passage which he now saw after it.

Yours faithfully,

Blackheath, 1888, Oct. 13. W. T. Lynn.

OBSERVATORIES.

Poona.—We have received the following letter from Mr. K. D. Naegamvala:—

Gentlemen,—

An observatory has lately been established at the College of Science, Poona, and we hope to start work before the end of the year.

The principal instruments are a 16½-inch silver-on-glass Newtonian by Sir H. Grubb, with a 4-inch finder attached, and a 6-inch equatorial refractor by the elder Cooke. Both these instruments are of the highest excellence, and besides eyepieces and micrometers they are furnished with several spectroscopes by Grubb, Hilger, Browning, and others. Photographic and photometric accessories of the telescopes are to be shortly procured. A transit instrument and a chronograph will also be soon obtained for the observatory.

It is intended at present to restrict the work of the observatory to certain branches of spectroscopic research, together with occasional observations of comets &c.

I beg, therefore, to appeal through you to all observatories, both public and private, to favour us with their publications for our Library. Copies and maps of spectra, photographs of celestial objects, &c.—in fact, all publications bearing particularly on *physical astronomy*—will be most thankfully received.

Perhaps the best and cheapest mode of sending presents to our Library will be through "the India Office, London."

Yours faithfully,

K. D. Naegamvala, M.A., F.R.A.S.,
Curator of the Observatory.

Wolsingham.—In Circular 22 Mr. Espin states that, according to Mr. Backhouse, ζ Herculis has increased in brightness, and that Mr. Gore confirms the observation. The Observatory has been removed to Tow Law, near Darlington, and is now 1000 feet above sea-level.

Ben Nevis.—We hear that among the proposals of a Committee appointed to consider the best means of disposing of the Edinburgh Exhibition surplus is a recommendation to give £1000 to the Scottish Meteorological Society in aid of the Ben Nevis Observatory.

PUBLICATIONS.

The Eruption of Krakatoa and Subsequent Phenomena.—The Krakatoa Committee has had a great deal of work to do, and has done it very well. Their Report was ready in MS. last year; but the printing has necessarily taken some time, owing to the large number of diagrams and tables which they have prepared, in addition to about 500 pages quarto of discussion. Although the subject is scarcely an astronomical one, the results of the discussion are so remarkable as to be of very general interest.

The Report is divided into five parts. The first deals with the actual volcanic phenomena. The eruption of Krakatoa in 1883 was apparently chiefly remarkable for the violence of one or two outbursts—notably that of August 27—which were far in excess of anything before recorded; the total amount of matter ejected was probably less than that of several previous eruptions. It is suggested that this violence was due to the inrushes of the sea, to which the volcano was accessible. Parts II., III., and V. deal with the waves which influenced the barometer, the tidal registers, and the magnetic registers respectively, and which were traceable to the eruption. The air-wave is remarkable for the marvellous accuracy of its observed oscillations. On a perfect sphere at rest, and with a perfect gas as atmosphere, the wave would expand from Krakatoa in a small circle to the great circle of which Krakatoa is pole; and then contract to the antipole, whence it would return again to Krakatoa in a similar manner. By a series of beautiful diagrams, General Strachey shows the gradual deformation of these theoretical circular ripples by the various disturbing causes; but the enormous distances which the wave travelled without deformation—or, in other words, the close accordance of the velocities of the wave deduced from the observed times of passage—are certainly surprising. At some places passages and re-passages of the wave were noted up to the seventh.

The sea-wave was traced to Cape Horn, and possibly to the *English Channel;* but this wave, which was 100 feet high near

Krakatoa, was soon reduced to a few inches, and is not readily distinguishable from other disturbing causes at the more distant ports. The hydrographer finds that the observed velocity is in all cases *less* than the observed depth of the sea would demand according to theory.

The magnetic waves were curious in character: the instruments suffered a gentle disturbance (nothing of an abrupt character), recognizable by the sequence of its occurrence at different places; ordinary magnetic disturbances being simultaneous all over the world. It is probable that this gentleness of the swing caused the wave to be overlooked at places from which Mr. Whipple has received the report "nothing unusual was noticed," and it would be interesting to re-examine these traces with the tabulated results of the Report. The magnetic wave appears to have travelled, roughly speaking, with the velocity of sound. Can this mean that it was the result of the air-wave in some way, which also travelled with that velocity?

From the evidence detailed in Part IV. there seems to be no doubt that the remarkable optical phenomena of the years 1883, 1884, 1885, and part of 1886 (coloured suns, twilight glows, sky haze, &c.) were due to a cloud of very fine dust ejected from Krakatoa to a height of about 25 miles, where it was caught by the prevailing current of that region, and carried rapidly right round the equator to the west, spreading comparatively slowly north and south of this belt. The sequence of the optical phenomena themselves affords the evidence of this current, about which we otherwise know nothing. The rate of fall of such particles as would produce the optical effects is about 10 miles in 2 years, as deduced from theoretical considerations; hence the long continuance of the phenomena. The height to which matter was blown from Krakatoa, too, agrees well with the height of the stratum as deduced from observations on twilight glows, assuming dust to have been the cause of these. The evidence for similar phenomena on occasions of other eruptions is also striking from its independent character.

The whole Report is well worth reading, though portions of it are rather long.

REPORT OF THE METEOROLOGICAL SERVICE OF THE DOMINION OF CANADA FOR THE YEAR ENDING DECEMBER 31, 1885. By CHARLES CARPMAEL, M.A., F.R.A.S., Superintendent.—This is the fifteenth annual report of the Meteorological Service of the Dominion of Canada. In addition to other meteorological information, the mean temperature of each month of the year, the highest and lowest temperature in each month, and the monthly mean daily range are given for a large number of stations. The mean daily temperature on *every* day of the year is also given, in all, for about one hundred *stations,* as well as five day means throughout the year for the same stations. Monthly results of observations of cloud and of registered bright sunshine are given also for a certain number of places. Rainfall tables (monthly values) are included for a large number of stations, and maps are added showing the distribution of rain and snow in different periods of the year. It is remarked that one inch of snow is taken as equivalent to one tenth of an inch of rain. Daily weather forecasts are issued throughout the year, and there is a storm-signal service. Forecasts of weather are asked for by many persons whose avocations are affected by changes in the weather.

Self-recording magnetographs were in operation throughout the year, but no results are contained in this volume. The argentic gelatino-bromide process of photography has been introduced in place of the old wax-paper process with great advantage. There is also a barograph, thermograph, and other self-recording instruments.

The work of preparing the observations taken at Toronto in connection with those of the International Polar Exploration stations has gone on but slowly, owing to the inadequacy of the staff. It is also mentioned that no regular and systematic astronomical observations (other than those for time &c.) have been as yet undertaken, but that preparations are making for regular spectroscopic observations of the solar flames. W. E.

NEW BOOK ON PLANETARY THEORY.—Dr. Otto Dziobek has just published an excellent book on planetary theory*, expounding in the clearest and simplest manner the researches of the great mathematicians who have worked at this subject. The book is in three parts, the first being devoted to the general problem of the motion of *n* bodies, with two and three bodies as particular cases; the second to the theory of the integration of the differential equations involved, in which the methods of Poisson, Lagrange, Hamilton, and Jacobi are examined; and the third to the theory of perturbations. The mathematics are elegant in form throughout—a virtue not too common in German works of the kind; and the thread of the argument is skilfully kept by short *résumés* of the various chapters. The note of this kind on perturbations is specially good. It may perhaps be mentioned that the book is distinctly theoretical, and only indirectly useful to the computer. The type and printing are good; we only notice a few trivial printer's errors:—on p. 46, line 13, 18) should be 20); on p. 87, line 11, 14 should be 17; and on p. 192, line 10 from bottom, the sign + is omitted. The thanks of others than the author are certainly due to Dr. von Gossler, whose generosity provided the endowment for the necessary researches.

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* Die mathematischen Theorien der Planeten-Bewegungen. J. A. Barth, Leipzig, 1888. Price 9*s*.

NOTES.

LATITUDE OF THE PARIS OBSERVATORY.—M. Perigaud has found that his improvements in mercury-baths have greatly facilitated the reflexion-observations of stars for latitude. He obtains for the latitude of Paris from 10 direct observations of Polaris above and below pole in 1888 October, combined with the nadir, 48° 50′ 12″·03, and from reflexion observations 48° 50′ 10″·76. The difference between the two results, which are uncorrected for flexure, disappears when his formula for flexure (0″·65 cosine zenith-distance), previously determined, is applied, the values becoming 48° 50′ 10″·89 and 48° 50′ 10″·92. He concludes (perhaps a little hastily) from the agreement of these values with that found in June last, that the determination is independent of the time of year, and may be regarded as final. The value given in the 'Nautical Almanac' for 1889 is 48° 50′ 11″.

The formula for flexure is a curious one; we are accustomed to the sine of the zenith-distance in such cases, but the present formula was found by M. Perigaud from a large number of observations of stars by reflexion, at zenith-distances varying from 10° to 70°; and he points out in explanation that in this particular instrument, the "Cercle de Gambey," the telescope is relatively small, and is attached to the circle at both ends, as usual in mural circles; and when horizontal the telescope acts as a binder to prevent the deformation of the circle by its own weight, whereas when vertical there may be an unsymmetrical displacement of the north and south portions. It would be well, as M. Faye suggests, to test this flexure correction independently by collimators. It would at any rate appear that the claim sometimes made on behalf of the mural circle that it was less subject to flexure may not be substantial.

THE ABSENCE OF OXYGEN FROM THE SOLAR ATMOSPHERE.—Another mountain expedition with an astronomical object has just been successfully undertaken. M. Janssen has given another proof of that enterprise in the cause of science which made him famous eighteen years ago, by taking his spectroscope up Mont Blanc as far as the Grands Mulets. He wished to compare the solar spectrum as seen through the comparatively small remnant of our atmosphere which lies above a mountain-station, with the same spectrum as seen from ordinary levels, in order to determine whether certain bands denoting the presence of oxygen had their origin in the solar atmosphere or in our own. If they were due to the former, they would remain unaltered by the ascent to a higher level: if to the latter, they should be enfeebled in proportion to the diminished thickness of the stratum of air through which the light passes. It was important to have the air as dry as possible, in order to avoid the interference of vapour-bands with the observations; and M. Janssen thus left his expedition till late in the year, when the temperature would be low, although he thus ran the risk of being delayed by bad weather. But he was favoured with two splendid days on October 15 and 16, immediately after the ascent, which enabled him practically to settle the question. The bands in question were distinctly feebler on Mont Blanc; most of them actually disappeared when the Sun was on the meridian. We must therefore refer these oxygen bands to our own atmosphere, and consider that as yet we have no evidence of the existence of oxygen in the solar atmosphere.

Personal Equation Machines.—Dr. Wislicenus of the Strassburg Observatory has arranged a very convenient and successful method of determining absolute personal equation in observing transits of stars and limbs, for instruments illuminated from the object-glass. In this method of illumination, light is reflected from a small disk on the inside of the object-glass to the eye end; and by placing a small convex lens behind the eyepiece, some of this light can be condensed into an artificial star in the plane of the wires. By moving the combination of eyepiece and lens across the field, the star is made to transit across the wires; and the absolute time of transit is registered on a chronograph by the interruption of a current when a fine point rigidly attached to the eyepiece and travelling in contact with a metallic surface passes over fine lines ruled on that surface and filled up with insulating substance. The advantage of this form of artificial star is that it is contained in the telescope and can thus be observed with the instrument in any position; whereas in almost all the previous forms of machine which Dr. Wislicenus describes in the early part of his paper * the instrument is necessarily horizontal. Experiments with his apparatus show (according to the author) that the position of the observer considerably affects the personal equation, as would appear likely; although M. Wolf came to an opposite conclusion after a few trials. The following are some of his results:—

Declination corresponding to rate of motion.	Position of Instrument				
	Zenith.	45°.	Horizon.	−45°.	Nadir.
°	s	s	s	s	s
11½	−0·19	−0·18	−0·21	−0·13	+0·14
60	−0·01	−0·02	−0·05	+0·04	+0·50
80	+0·52	+0·48	+0·32	+0·41	+1·77

It must be remembered, however, that in different positions of the telescope the contacts of the recording apparatus might be sensibly different. Very little information is given as to the adjustment of this apparatus.

* Untersuchungen über den absoluten persölichen Fehler bei Durchgangsbeobachtungen.

By employing discs of different sizes placed similarly behind the eyepiece, Dr. Wislicenus has also investigated his personality in observing limbs of planets.

Reflected Image of the Sun on the Sea Horizon.—At the meeting of the Paris Academy on Oct. 8 M. Ricco communicated the result of observations made at Palermo, since July 1886, at about 1¼ miles from the shore and 236 feet above sea-level, the dip of the horizon being 15′. Photographs were taken with a 4½-in. Merz telescope, the finder being used for eye observations. M. Ricco proposes to continue the observations next year; but from those already made he finds that with a clear sky and calm sea the elliptical form of the sun's image is very marked, and he is surprised that ancient astronomers did not perceive in this phenomenon an indication of the rotundity of the earth.

M. Wolf has computed the magnitude of the deformation. He supposes an observer stationed 328 feet above the sea, for which altitude the dip is 19′·8, to view directly a luminous point whose angular altitude above the apparent horizon is α, and from this he computes the angular altitude ω above the same horizon of its image reflected from a curved mirror of radius 21¾ miles, in form like that of the surface of a calm sea. For example

α	35′·1	30′·3	26′·2	22′·7
ω	10·7	7·9	5·8	4·3
$\alpha-\omega$	24·4	22·4	20·4	18·4

When the sun's lower limit is tangential to the horizon, the vertical diameter of the reflected image is reduced about 10′, a result in accord with the observation of M. Ricco.

Mercury-baths and Artificial Horizons.—It is well known that the stability of a stratum of mercury is rapidly increased as the thickness of the film is diminished. Where steadiness is of great importance, as in reflection observations, different methods may be used to give the requisite tenuity to the film, that most commonly employed being to amalgamate the surface on which it rests. For small baths this method is certainly very satisfactory, and will give good results in cases where otherwise any observation at all would be impossible. But the mercury is apt to get dirty quickly, and though it can be readily cleaned by passing moistened paper over the surface, this constant cleaning has been found troublesome in the case of large instruments. In consequence a different method has been adopted for a large meridian instrument at Paris. A trough is firmly fixed in the ground, the bottom of the trough being accurately levelled and carefully cleaned. A small stratum of mercury is poured over this surface and, by opening a small hole, the quantity is gradually reduced until an extremely thin film is left—in a state of tension, but still presenting a perfectly horizontal surface of great stability. At the slightest touch the film breaks and collects into a single globule. This method, due to M. Perigaud, can only be used for fixed troughs.

We do not understand why the amalgamated trough has not been used more extensively in sextant observations. There must be many cases where it would be found very useful; and we should imagine that the glass covers to the artificial horizon, often a source of trouble and sometimes of error, could perhaps be dispensed with if a really stable film were used.

THE METEORITIC THEORY.—At the meeting of the Royal Society on Nov. 15, Prof. G. H. Darwin read a remarkable paper dealing with the "Mechanical Conditions of a Swarm of Meteorites" from a mathematical standpoint. Remarking that the very essence of the nebular hypothesis is the conception of fluid pressure, without which the idea of a figure of equilibrium becomes inapplicable, he devotes his paper to reconciling with this conception the notion of a meteoric condition of matter, which at first sight seems directly inconsistent with it. To quote his own words, "according to the kinetic theory of gases, fluid pressure is the average result of the impacts of molecules. If we imagine the molecules magnified until of the size of meteorites, their impacts will still, on a coarser scale, give a quasi-fluid pressure. I suggest then that the fluid pressure essential to the nebular hypothesis is, in fact, the resultant of countless impacts of meteorites." For the development of this view three steps are important. We must first demonstrate the analogy between the behaviour of the individual meteorite and that of the individual molecule. Secondly, this analogy established, we can lose sight of the individual meteorites, and apply the known results of the theory of gases to a swarm of meteorites with such modifications as are traceable to the modified behaviour of the individual. Thirdly, to brand the hypothesis as something more than a mere speculation, it is necessary to apply some numerical test derived from the consideration of some actual system, the solar system being obviously the only one available.

With regard to the first point, the chief difficulty is in respect to impact. The molecules in the kinetic theory of gases are to be perfectly elastic; but "when two meteoric stones meet with planetary velocity the stress between them during impact must generally be such that the limits of true elasticity are exceeded." But the enormous velocity of approach will volatilize a portion of each, and thus impart what virtually amounts to a nearly perfect elasticity. In direct collisions each stone is probably shattered; but direct collision must be a comparatively rare event. For glancing collisions, the nearest terrestrial analogy is when a cannon-ball rebounds from the sea. The degradation in size which would result from such fractures as do occur is probably compensated by a condensation of the vapour liberated at impacts round the meteorites, and a collection by molten meteorites of smaller fragments. Since the number of fractures depends on the average size and velocity of the swarm, a condition of ultimate stability will involve a relation between this average size and average velocity.

Later it is shown, too, that the near approach of two meteorites has no appreciable influence on the straight path of either, the mutual attraction of such small bodies being insensible; and hence nothing but an actual impact is to be treated as a collision.

The theory of gases as applied in the second step is to consider the properties of an "isothermal adiabatic sphere," an arrangement of spherical symmetry where the density decreases outward from a central nucleus so as to represent an atmosphere in convective equilibrium, this law of decrease of density being determined in the paper.

In the third step the important point to be examined is whether the analogy with the theory of gases will hold for a swarm of meteorites of the size necessary to form our solar system, and it is shown that the analogy still holds for a swarm from which the Sun may be supposed to have been formed when this swarm extended beyond the orbit of the planet Neptune.

The theory being thus found to be applicable, some of the results are detailed. It appears that a widely dispersed swarm would aggregate first to the arrangement called an "isothermal adiabatic sphere," consisting of a central nucleus and an atmosphere for which two different distributions of density hold. In so doing, half the potential energy of separation of the meteorites is expended in volatilizing solid matter, and gradually lost by radiation; the other half remains as kinetic energy of agitation. Later the central nucleus disappears, the law of density becoming uniform throughout, viz. that of a sphere of gas under the influence of its own gravity. This law does not hold, however, near the fringe. Throughout the swarm the meteors are to some extent sorted according to size, the large ones being towards thecentre. The swarm is subject to gaseous viscosity, especially when widely diffused; and hence will tend to rotate as a solid body when sufficiently diffused, though when more contracted the central portion will revolve more rapidly than the outside.

LUNAR RAINBOW.—Mr. W. E. Stewart, of Croft-on-Tees, Darlington, informs us that he observed a fine lunar rainbow on Nov. 19. The Moon was full on Nov. 18, and rose on Nov. 19 at 17^h. The bow was visible for more than an hour.

THE SATELLITE OF NEPTUNE.—M. Tisserand points out (C. R. No. 21, p. 810) that the changes in the plane of the orbit of this satellite, to which Mr. Marth called attention in 1886[*], can be

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See 'Observatory,' No. 102, p. 346, and No. 142, p. 413.

explained by a small ellipticity in the figure of the planet, which would escape observation at the great distance of Neptune. The magnitude of this ellipticity (ϵ) depends on the assumed inclination (C) of the planet's equator to the orbit of the satellite, which remains constant. M. Tisserand finds that for

$$C=20^\circ,\quad \epsilon>\tfrac{1}{85},$$
$$C=25\ ,\quad \epsilon>\tfrac{1}{102},$$
$$C=30\ ,\quad \epsilon>\tfrac{1}{118},$$

the inequalities becoming equalities if the planet is homogeneous, and increasing as the mass is more condensed towards the centre. The period of rotation of the plane of the orbit is

$$(\text{no. log } 3{\cdot}177)\sin C,$$

and is more than 500 years. It remains to confirm this hypothesis, and to find C, by subsequent observations.

COMET *e* 1888 (Barnard).—Dr. L. Becker has calculated the elements of this comet by Variation of the Distances, and finds that the most probable orbit is hyperbolic.

Ephemeris for Greenwich Midnight *.

1888.	R.A. h	m	s	Decl. °	′	Log *r*.	Log Δ.
Dec. 3....	2	3	9	−6	41·4	0·2928	0·0669
7....	1	40	50	7	12·2	·2887	·0887
11....	1	21	15	7	31·5	·2847	·1131
15....	1	4	21	7	41·2	·2810	·1390
19....	0	49	59	7	43·4	·2776	·1654
23....	0	37	47	7	40·1	·2744	·1916
27....	0	27	29	7	32·8	·2715	·2170
31....	0	18	49	−7	22·1	0·2689	0·2414

Mr. Sadler points out that this comet will, according to the above ephemeris, transit almost centrally the bright nebula ♅ I. 100 Ceti (G.C. 342, Dreyer 584) a short time before midnight on Dec. 10. The faint nebula ♅ III. 431 (G.C. 345·6, Dreyer 586) will also be occulted. Observations of these phenomena, especially with the spectroscope, would be of considerable interest.

M. O. CALLANDREAU, well known by his theoretical researches on external figure and internal constitution of celestial bodies, remarks that, like the moment of inertia, the potential energy of gravitation, W, of a planet

$$W=\tfrac{1}{2}\int V\,dm$$

(V is the potential of an ellipsoid on the element *dm*) may be very approximately expressed by a function of the planet's dimensions, mass, and angular velocity of rotation, without regard to the law of internal density.

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* Dun Echt Circular, No. 164.

He supposes, as for calculating the moment of inertia, that compressibility decreases rapidly with pressure, and bases his calculation on Clairaut's well-known equation, assuming the form given to it by M. Radau.

He obtains thus for the Earth:—

$$W = 0{\cdot}654\frac{M^2}{a},$$

where M is the mass, a the equatorial radius.

We regret to hear of the death of Mr. John Rand Capron, of Guildown, Guildford, who has been for some years a prominent member of the Royal Astronomical Society. He was on the Council from 1883 to 1887. His most important astronomical work has been in connexion with auroræ, and his "Auroræ, their Character and Spectra," published in 1879, is a standard book on the subject. He has also published "Photographed Spectra: one hundred and thirty-six photographs of metallic, gaseous, and other Spectra;" and has contributed many papers to the Society. His health had not been good for some time, and he became seriously ill towards the end of October. He died on November 13 at the age of 59 years, leaving a widow, but no family.

Among interesting lectures to come, we notice a course of four on "Light and Colour," to be given by Capt. Abney to the Society of Arts; and another of four to the same Society by C. V. Boys, F.R.S., on "Instruments for the Measurement of Radiant Heat." Those who attend these lectures are almost sure of seeing some exquisite experiments.

We learn that "the largest telescope in the world is not yet built." It is not, however, to be erected in Ireland, but in Southern California. The object-glass is to be 42 inches in diameter, at £500 an inch. Messrs. Alvan Clark expect to build it in about five years.

Errata in No. 142:—P. 414, line 14 from bottom, *for* "instead of decreasing, increases slightly," *read* "instead of increasing, decreases slightly." Dr. de Haerdtl's name should be spelt with a *d*.

Minor Planets Nos. 280 and 281 were discovered by Dr. Palisa on Oct. 29 and 31.

The Rumford Medal of the Royal Society has been awarded to Prof. P. Tacchini for his researches in Solar Physics.

Mr. A. Cowper Ranyard will finish the 'Old and New Astronomy,' which was not quite complete at the death of Mr. Proctor.

Comet f 1888 was discovered by Mr. Barnard on Oct. 31.